BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded by G. Stanley Hall in 1887

Vol. XXX APRIL, 1919 No. 2

A STUDY OF TONAL ATTRIBUTES[1]

By Gilbert J. Rich

TABLE OF CONTENTS

	PAGE
Introduction	121
I. Historical Review	122
II. Observers, Apparatus and Procedure	127
III. Vocality	131
Preliminary Training	131
Method of Paired Comparisons	133
Vocal Limens	137
Discussion	142
IV. Pitch	143
V. Volume	149
VI. Brightness	153
VII. Tonality	158
VIII. Criticism of Previous Experimental Work	162
Conclusions	163

The pendular vibrations which form the simplest sound-waves of physics, and which are correlated with the tonal sensations of psychology, have the characteristics of amplitude and frequency. We were formerly taught that the amplitude of the wave corresponds, on the side of sensation, to the single attribute of intensity, and the frequency of the wave to the single attribute of pitch. But if we examine tonal sensations closely, we find a number of psychological characteristics. One may say, for example, that a given tone is high or low, large or small, bright or dull; that it is the musical note *c* or *d*, or that it resembles the vowel O or the vowel U. Is every one of these judgments based on a

[1] From the Psychological Laboratory of Cornell University.

separate attribute of the tonal sensation? Do some among them represent different attitudes taken up toward the same attribute? Or are certain of them made upon an integrative or associative rather than upon an attributive basis? These questions constitute the problem of the present study.

I. Historical Review

The problem, as such, is of recent date. The observations that give rise to it are, to be sure, much older. Stumpf[2] shows that the similarity of tones standing an octave apart is recognized both in music and in the earlier psychologies. The similarity of certain tones to vowels also receives early recognition. Willis,[3] in 1828, says that "vowels are a different affection of sound from both pitch and quality, and must be carefully distinguished from them." Hensen,[4] in 1891, is another observer who notices the specific vowel-qualities of certain tones. It is a common observation that low tones are large and massive as compared with higher tones. Mach[5] holds that all tones are composed of varying proportions of two elements, the one bright and the other dull.

The systematic treatment of these characteristics in relation to attributes starts with Stumpf. In the first volume of his *Tonpsychologie*, he discusses the characteristics of volume[6] and brightness,[7] denying that either is an attribute, and holding that both are matters of association only. But in the second volume of the same work,[8] some seven years later, he takes the position that tones possess an attribute of extension or volume, parallel to pitch. Volume, pitch, and intrinsic intensity (with which we are not here concerned) form together his *Tonfarbe*. Brightness, however, he does not consider a separate attribute. Nor does he admit any attributive basis for the similarity of the octave, which is for him based on degree of fusion.[9]

McDougall[10] separates pitch into two attributes. The one of these, quality, is common to the given tone and to all its upper and lower octaves; so that all the qualities of tonal sensation are contained within a single octave. The other attribute, which distinguishes the same tone in different octaves, is "of the same order as differences of extensity in the case of visual, tactual, or temperature sensations."

M. Meyer,[11] in 1903, postulates two attributes of tones, that change with the physical frequency. These he calls *Tonhöhe* and *Tonfarbe*

---

[2] C. Stumpf, Über neuere Untersuchungen zur Tonlehre, *Ber. u. d. 6 Kong. f. exper. Psych.*, 1914, 309 f.

[3] R. Willis, On Vowel Sounds and on Reed-Organ Pipes, *Trans. Camb. Phil. Soc.*, 3, 1828, 231 ff.; Über Vocaltöne und Zungenpfeifen, *Ann. d. Phys. u. Chem.* (Pogg.), 24, 1832, 415 ff.

[4] V. Hensen, Die Harmonie in den Vocalen, *Zeit. f. Biol.*, 28, 1891, 46.

[5] E. Mach, *Beiträge zur Analyse der Empfindungen*, 1886, 121 f.; cf. Stumpf, *Tonpsychologie*, ii, 1890, 272 ff.

[6] C. Stumpf, *Tonpsychologie*, 1, 1883, 207 ff.

[7] *Ibid.*, 221.

[8] *Ibid.*, 2, 1890, 56; 336; 535; 539.

[9] *Ibid.*, 199; 407 ff.; Konsonanz und Dissonanz, *Beitr. z. Akus. u. Musikwiss.*, 1, 1898, 45 ff.

[10] W. McDougall, *Physiological Psychology*, 1899, 72 f.

[11] M. Meyer, Zur Theorie der Geräuschempfindungen, *Zeit. f. Psych.*, 31, 1909, 247.

(pitch and quality, in English). *Tonhöhe* is lacking in noises. In the following year, he enlarges upon the difference between the two attributes.[12] The difference between tones and noises, he holds, can be understood only if we assume that one attribute, namely pitch, is lacking in noises; for if we can say that one noise is higher than another, we cannot name the interval between them. Similarly, very high and very low tones, which are often called noises, can be judged only in terms of quality. Music, then, depends upon pitch, to which unmusical persons are deaf. The similarity of the octave, Meyer further says, can be explained by the assumption of two attributes of tone; but the details of such an explanation are not worked out. Later, after Révész and Köhler have published their work, Meyer[13] insists that his pitch and quality are identical with Révész' *Qualität* and *Höhe* respectively, and his quality with Köhler's *Vokälitat*.

Dunlap[14] (1905, amplified 1912) calls attention to the common observation that low tones are large or voluminous. "Differences in pitch," he says, "are directly comparable to differences in planar linear extent." He therefore designates pitch as the extensive attribute of tone, excluding any attributive multiplicity.

Brentano[15] (1907) posits two tonal attributes. The one, *Qualität*, recurs at every octave; the other, *Höhe*, is composed of bright and dull factors, which unite in continuously varying degrees to form every tone in the scale.

In 1908, Titchener[16] distinguishes two qualitative attributes of tones, pitch and volume. Tones vary in volume, the low tones being large and massive, the high tones shrill, thin, and sharp. This difference is not spatial. Volume is not an intensive attribute, but a qualitative attribute, moving between the extremes of mild and shrill. Volume and pitch, moreover, are independently variable, in the sense that at the two ends of the scale volume changes more quickly than pitch, while over the middle region it changes more slowly.

The next contribution to the problem is that of Köhler (1909, 1910, and 1913), who deals with vocality. We are not ourselves concerned with the vowel-theories with which he principally deals. In his first paper,[17] Köhler records the observation that the tones of certain tuning-folks sound like the different vowels. His second paper[18] reports a continuation of experimental work along the line thus suggested. In the preliminary experiments the observers are required to report what vowels the tones of various tuning-forks resemble. They find that some resemble the pure vowels U, O, A, E, and I, while others stand between two of these vowels. This similarity appears to be something given directly in the tones and not due to association;

---

[12] *Id.*, On the Attributes of the Sensations, *Psych. Review*, 11, 1904, 83 ff.

[13] M. Meyer. Review of Révész, *Psych. Bull.*, 11, 1914, 349 ff.; Vorschläge zur akustischen Terminologie, *Zeit. f. Psych.*, 68, 1914, 115 ff.

[14] K. Dunlap, Extensity and Pitch, *Psych. Rev.*, 12, 1905, 287 ff.; *A System of Psychology*, 1912, 123 f.; 143 f.

[15] F. Brentano, *Untersuchungen zur Sinnespsychologie*, 1907, 101 ff.

[16] E. B. Titchener, *The Psychology of Feeling and Attention*, 1908, 12 ff.; *A Text-Book of Psychology*, 1911, 94 ff.

[17] W. Köhler, Akustische Untersuchungen, I, *Zeit. f. Psych.*, 54, 1909, 283 ff.

[18] W. Köhler, Akustische Untersuchungen, II, *Zeit. f. Psych.*, 58, 1910, 59 ff.

but the introspective data are scanty. In the main experiment, the observers make similar reports of the vocality of 30 forks presented 15 times in haphazard order. The reports show, on the average, a regular progression of the vowel qualities up the scale in the order U, O, A, E, I, though sometimes an Ä is found between A and E. Tones lying between two of the pure vowels are like both of the adjacent vowels. On the basis of these results, Köhler postulates a qualitative attribute of tones, alongside of pitch, which he calls *Qualität, Vokalqualität,* and later *Vokalität* (vocality). He next attempts to find the turning points (*die ausgezeichneten Punkte*) of the phenomenological system of vocalities. Working with pure tones, he starts with a 'mixed' vowel and changes the frequency of vibration until the observer reports a pure vowel, continuing until a trace of the vowel next beyond is heard; a number of series is taken in both directions. The whole set of turning points from the semi-vowel M, which Köhler finds below U, to I are in almost exact octave relations. In a third paper,[19] Köhler considers the vocality of very high tones. At succeeding octaves above I he hears S, F, and Ch, and insists that these sounds are tones, not merely noises.[20]

Révész, in 1912 and 1913,[21] observes that there are two ways of taking the tones which stand an octave apart. If we take them in the one way, they are the most dissimilar tones within the octave; if we take them in the other way, they are the most similar. This latter similarity he calls 'octave-similarity.' He therefore presents to his observers series of pure tones, asking them which are the most similar, and finds that the octaves are so judged. Treating these phenomenological observations on the systematic side, Révész posits two attributes of tonal sensation. That which recurs at every octave he calls *Qualität* (quality); that which is different in different octaves he terms *Höhe* (pitch). He then proceeds to show that these two attributes are independently variable. In the first place, two tones can be judged as different when their physical frequencies differ so slightly that the direction of the difference cannot be stated. Since such a judgment is a judgment of quality but not of pitch, the differential limen is lower for quality than for pitch. Secondly, the same quality occurs with different pitches in the case of two tones standing an octave apart. Thirdly, Révész has discovered a pathological subject for whom quality is displaced in the one ear for a certain range of the scale, without any corresponding displacement of pitch. Fourthly, tones near the upper and lower limits of hearing, as well as the sensations of melody-deaf persons, lack quality though they possess pitch; for it can be said that one such tone is higher than another, but not that it is the octave of the other. Révész accepts Köhler's attribute of vocality, but does not identify it either with his pitch or with his quality.

Stumpf reviews the work of Révész and Köhler before the Sixth

---

[19] W. Köhler, Akustische Untersuchungen, III and IV, *Zeit. f. Psych.*, 64, 1913, 92 ff.

[20] It would seem that Köhler means by 'tone' any sensation aroused by a regular periodic sound wave, and not a particular form of experience.

[21] G. Révész, Nachweis dass in der sog. Tonhöhe zwei von einander unabhängige Eigenschaften zu unterscheiden sind, *Nachr. königl. Gesell. Göttingen*, 1912, 2, 247 ff.; *Zur Grundlegung der Tonpsychologie*, 1913.

Congress for Experimental Psychology.[22] He accepts Révész' distinction of quality and pitch as attributes of tone, and specifically identifies the impressions of pitch and brightness. But he criticizes a number of the proofs brought forward by Révész in the attempt to show independent variability. The conclusions drawn from the pathological case, as well as the statement that the differential limen is lower for quality than for pitch, go further than the facts justify. Other interpretations are possible; for an observer should be able to tell the direction of a quality-difference as well as the direction of a pitch-difference. Stumpf tries some of the experiments of Köhler for himself, and fails to find a regularity of judgment. Some of the observers cannot distinguish any pure vowels in tones, while others can hear only U and I. As all of Köhler's pure vowels come at the note *c*, Stumpf thinks it possible that Köhler's observers came to call the quality *c* a pure vowel, a turning point in the tonal system. The different vowels, then, are simply the same quality with different pitches or brightnesses; O is a bright U, and so on. Thus Stumpf declines to admit vocality to his list of attributes, of which he now has three: namely, *Qualität*, *Helligkeit* (brightness), and volume.

Watt[23] (1914) is unwilling to accept Köhler's work on the vowel-qualities, which he considers as apart from the tonal series. He does, however, accept Révész' distinction of two attributes, which he calls pitch and volume. His pitch is equivalent to the *Qualität* of Révész; his volume is an attribute which changes continuously over the scale, and which he identifies with the *Höhe* of Révész. He supports it by citing the usual phenomenological observations of the voluminous character of the tonal series.

In his last paper[24] (1915), Köhler takes up the problem of the tonal attributes as such, and starts with a series of definitions. *Tonhöhe* (which we shall translate as 'musical pitch') is that attribute by which tones are named and intervals judged. 'Brightness' is used for auditory phenomena which give the impression of brightness; a further characterization is not possible. 'Vocality' is used as in his earlier works. Brightness and vocality taken together are designated as *Tonkörper* (tone-body). Volume and intensity also belong to the tone-body, but are irrelevant to the discussion. Köhler now cites instances to show that musical pitch and tone-body are independent. In the upper range of the musical scale judgments of interval are displaced, while the turning-points of the vocality-series are not. In the pathological subject on whom Révész experiments, the octave-relation of the pure vowels is normal within the affected region, so that only musical pitch (and not tone-body) is displaced. Moreover, very high tones, very low tones, and the tonal sensations of unmusical subjects lack musical pitch, while tone-body is present.

Köhler's main experiment (1910) on vocality is repeated by Modell and Rich,[25] with the variation that a serial instead of a haphazard order is used for presentation of the stimuli. The results are similar

[22] C. Stumpf, Über neuere Untersuchungen zur Tonlehre, *Ber. u. d. 6 Kong. f. exper. Psych.*, 1914, 305 ff.

[23] H. J. Watt, Psychological Analysis and the Theory of Hearing, *Brit. Jour. of Psych.*, 7, 1914, 1 ff.

[24] W. Köhler, Akustische Untersuchungen, V, *Zeit. f. Psych.*, 72, 1915, 1 ff.

[25] J. D. Modell and G. J. Rich, A Preliminary Study of Vowel Qualities, *Amer. Jour. of Psych.*, 26, 1915, 453 ff.

to those of Köhler, save that the exact octave-relation between the pure vowels is not found. Weiss[26] reports similar experiments with variable results. He attempts to identify vocality with Titchener's attribute of volume.

In 1916, Rich[27] publishes a study of the attribute of volume. His method of attack is a determination of the differential limen for volume-judgments at six points on the tonal scale. The results show that judgments of volume can be made with ease and readiness, and that they appear to be made upon an attributive basis. The limen for volume is everywhere higher than the pitch-limen; moreover, it tends to follow Weber's Law. Volume, therefore, satisfies the two criteria of inseparability and independent variability. The results, however, are obtained with tones from variators, and require verification with pure tones.

Watt, in 1917,[28] attempts to build up tonal psychology on a new basis. He rejects vocality, as lying outside the tonal series, and octave-quality as being unnecessary for purposes of explanation. He accordingly postulates but two attributes that vary with the physical frequency, pitch and volume. Neither of these is a qualitative attribute; for all sounds have one and the same 'quality.' Watt conceives of pitch as an ordinal attribute, an attribute of position, and of volume as an extensive attribute. Every phenomenological tone (except the very highest) is the sum of a large number of orders. The tonality of these orders constitutes the volume of the tone, while the predominant order gives the tone its characteristic pitch. The volume of any tone includes the volumes of all tones above it, for the volumes are planar and have, longitudinally, a common starting-point at the order of the highest audible tone. In support of his admission of volume

TABLE I

Author	Bright—Dull	High—Low	Large—Small	Vocal	Octave-Similar
Stumpf..(1890)	P..i..t..	..c..h.....	Volume		
McDougall....		V..o..l...	.u..m..e		Pitch
Meyer.....	Q..u..a..	.l..i..t..y		Quality	Pitch
Dunlap.....		P..i..t..	..c..h		
Brentano.....	Pitch				Quality
Titchener.....		Pitch	Volume		Tonality
Révész.....		Pitch		Vocality	Quality
Stumpf..(1914)	Pitch..or..	.Brightness	Volume		Quality
Watt... (1914)		V..o..l..	.u..m..e		Pitch
Köhler.....	Brightness		Volume	Vocality	Pitch
Watt....(1917)		Pitch	Volume		
Ogden.....	Brightness	Pitch	Volume		

[26] A. P. Weiss, The Vowel Character of Tuning-fork Tones, *Psych. Rev.*, 12, 1915, 63 f.

[27] G. J. Rich, A Preliminary Study of Tonal Volume, *Jour. of Exper. Psych.*, 1, 1916, 13 ff.

[28] H. J. Watt, *The Psychology of Sound*, 1917.

as an attribute, Watt quotes the common observation that low tones are large and high tones sharp and thin; notes that the discrimination of pitch is finer than that of volume in the middle of the scale and coarser at the extremes; cites the implication of an extensive attribute inherent in his concept of pitch as order; and states the need of such an attribute for the explanation of fusion. Watt further holds that the judgment of intervals is based on the ratio of the volumes of the two tones. In particular, a given tone has twice the volume of its octave, so that as we go up the scale by octaves the volumes of the tone do not decrease by equal amounts, but are approximately halved.[29]

Table I shows how the attributes postulated by the various writers fit together, those attributes which are similarly defined being listed in the same column. In the first column are to be found the attributes resulting from the judgments "brighter" and "duller," or identified with such judgments. The second column contains the attributes described by the terms "higher" and "lower." The attributes listed in the third column are typified by the judgments "larger" and "smaller." In Column 4 are attributes characterized by the similarity of their tones to some vowel or vowels, as well as proposed attributes later identified by their author with this characterization. Finally, in the last column is the attribute which has been defined only as being constant for a given note in different octaves. In recent works in English this characteristic has been called *tonality*.[30]

II. Observers, Apparatus and Procedure

Nine observers took part in our experiment: Dr. R. M. Ogden (O), professor of education; Dr. H. P. Weld (W), assistant professor of psychology; Dr. K. M. Dallenbach (Da), instructor in psychology; Mr. F. L. Dimmick (Di), assistant in psychology; Miss J. M. Gleason (G), Miss C. L. Friedline (F), Mr. P. T. Young (Y), and Mr. C. M. Clark (C), graduate students in psychology; and Mr. E. de Laski (L), an undergraduate 'majoring' in psychology. Of these observers, O, W, and Da were highly trained in psychological observation; Di, G and Y were relatively well trained; while the remaining three, F, C, and L had had comparatively little training. O, W, Y and C were markedly musical; the others, except F, were sufficiently musical to play some instrument or to sing. F turned out to be a typically 'unmusical' or 'pitch-deaf' subject. This fact was not discovered for some time. Special work was then undertaken, but unfortunately could not be completed. The results obtained with F in the regular series are, of course, valueless: most of the differences employed were considerably below her limen for any form of tonal discrimination. L was peculiar in that he seldom gave judgments of equality, even when the stimuli were objectively equal. The results obtained from

[29] The idea that the volume of a tone decreases by halves as the tone becomes an octave higher implies, mathematically, that volume cannot follow Weber's Law: a direct contradiction of the results obtained by Rich.

It is worth noting that the whole of Watt's treatment of the attributes is in the interest of their explanatory value for fusion, not in the interest of tonal analysis.

[30] M. Meyer, review of Révész, *Psych. Bull.* 11, 1914, 349; E. B. Titchener, *A Beginner's Psychology*, 1915, 52 (where the attribute of tonality is accepted). The list of attributes given by R. M. Ogden (*Psych. Rev.*, 25, 1918, 227 ff.) is considered at the end of this paper.

him are valueless for our purposes, since they do not fit the *phi-gamma* hypothesis (see p. 131 below); they were therefore discarded. All the observers knew the general nature of the problem and were acquainted with earlier theories on the subject. Not all were utilized in every set of determinations; considerations of time frequently prevented.

As sources of tone we had three Stern variators: No. 2, 150-300 vs; No. 4, 300-600 vs; and No. 7, 650-1200 vs. The bottles were blown from a compressed-air system kept at a pressure of 14.6 mm. of mercury. This pressure was too great for use at the mouths of the bottles, and was reduced by a system of valves and pinch-cocks. The actual pressure at the mouths differed from time to time, as it was found necessary to reset the mouths and the pressures (accompanied by a retuning) to meet the needs of different parts of the experiment. The variators were tuned, by comparison with standard Koenig forks, for a temperature of 21° C. The maximal deviation of the experimental room from this temperature was only 5°, giving a maximal temperature-error of 1%. The average error was, of course, much smaller.

To obtain pure tones a set of 24 interference-tubes was employed. Krueger[31] has shown that maximal interference is obtained when the interference-tubes are so placed along the conducting tube that they stand at the nodes of the tone to be eliminated, an integral number of half wave-lengths apart. Most interference-tubes are built to satisfy this requirement only for one region of pitch and its octaves. We desired, however, a more generally serviceable piece of apparatus, that could be adapted for any pitch by adjustment of the distance between the interference-tubes. Accordingly, every interference-tube was mounted separately on a short section of conducting tube (10.1 cm.). Any length of pipe could then be laid between two of these units. The junctions between the sections of the conducting tube were formed by hardwood blocks, bored exactly to accommodate the pipe. The blocks ran upon a hardwood track, and could thus be set readily in the required positions. The conducting tubes and interference-tubes were of brass, of the same diameter, 19 mm. The interference-tubes were fitted with pistons, the maximal length obtainable being 55 cm.[32]

In setting up this apparatus, we tried at first to use only two rooms, but found it impossible to do so because the brass piping tended either to absorb or to give out sound-waves. When the interference-tubes were placed in the same room with the variator, they absorbed sound-waves from the air of the room; so that a tone eliminated by the earlier tubes might be picked up and transmitted by those further along. On the other hand, when the tubes were placed in a separate room from the sources of sound they gave off sound-waves to the air of this room; so that it was impossible to keep the observers and the tubes in the same room.

Three rooms were therefore used in the experiment. The variators were placed in the first room, before the open end of the tube, over which was fitted a paper funnel, 17 cm. in diameter and 21 cm. long. The sounding variator was about 2 m. from the wall through which the tube passed, and about 1.5 m. from the nearest side-wall. On both the side-wall and the wall behind the variator curtains were hung to minimize reflection. The conducting tube ran from this room through

[31] F. Krueger, Zur Theorie der Combinationstöne, *Phil. Stud.*, 17, 1901, 224.

[32] The apparatus was made to Dr. Titchener's specifications by the C. H. Stoelting Co., of Chicago.

a stone wall approximately 55 cm. thick to the middle room, which contained the interference-tubes. The track mentioned above was placed on long tables, and the interference-tubes were set in blocks running along the track. Beyond the interference-tubes the conducting tube passed through a double plaster wall to the room in which the observers sat, and ended flush with the further surface of this wall.

Since it was desirable to work with several observers at the same time, three booths were built in the observers' room. The observers sat facing the wall through which the pipe came. A three-way distributor, set in a rubber stopper, was placed in the end of a conducting tube, and the sound was led to the observers by rubber tubing, which terminated in pairs of ear-tubes of the type furnished with the dictaphone. The 'ready' and 'now' signals were given by a muffled bell, fastened to one of the booths, which was readily heard by the observers when the ear-tubes were in position.

All the stimuli of the experiment were drawn from three regions whose limiting tones were octaves of one another. If, now, the interference-tubes were so placed along the conducting tubes as to lie an integral number of half wave-lengths apart for the overtones of the lowest region we were using, then they must also be at the same time an integral number of half wave-lengths apart for the overtones of octaves of our lowest fundamentals. It was, therefore, not necessary to change the distances between the interference-tubes during the course of the experiment. The first interference-tube was 244.5 cm.

TABLE II

LENGTHS OF INTERFERENCE-TUBES IN CM.

Tube–number	Set for partial	235–295 vs.	470–590 vs.	940–1180 vs.
1	2	14.3	7.1	3.1
2	4	7.1	3.1	1.4
3	2	15.0	7.4	3.35
4	4	7.4	3.35	1.5
5	2	15.8	7.7	3.6
6	4	7.7	3.6	1.6
7	2	16.5	8.0	3.8
8	4	8.0	3.8	1.7
9	2	17.3	8.3	4.05
10	4	8.3	4.05	1.8
11	2	18.1	8.6	4.3
12	4	8.6	4.3	1.9
13	3	9.2	4.5	2.05
14	3	9.7	4.75	2.2
15	3	10.2	5.0	2.35
16	3	10.7	5.25	2.5
17	3	11.2	5.5	2.65
18	3	11.7	5.75	2.8
19	5	5.5	2.4	.95
20	5	5.75	2.6	1.05
21	5	6.0	2.75	1.15
22	5	6.25	2.9	1.3
23	5	6.5	3.1	1.4
24	5	6.75	3.3	1.5

from the end of the conducting tube next the variators.[33] The first twelve interference-tubes, set alternately to cut out the second and fourth partials, were 16.2 cm. apart. They were followed by six tubes, 21.7 cm. apart, set for the third partial. The remaining six tubes, set for the fifth partial, were 21.6 cm. apart. From the last interference-tube, the pipe ran 242.2 cm. to the rubber stopper in which was set the three-way distributor.

We took advantage of the finding of Köhler[34] that it is possible to set a series of interference-tubes in such wise as to secure pure tones over a considerable range. To accomplish this result, six interference-tubes designed to absorb the same partial were set at slightly different lengths. Our tubes were set to give pure tones throughout the three ranges: 235 to 295 vs., 470 to 590 vs., and 940 to 1,180 vs. The various lengths of the interference-tubes are shown in Table II.[35] The method of their determination can best be shown by an example, *e.g.*, the second partial of tones in a range 235 to 295 vs. The second partial of 235 vs. is 470 vs. A tone of this frequency was sounded, and one of the interference-tubes was shifted in and out until the point of maximal absorption was found. A number of trials was made, and the results were averaged. The same procedure was followed with a tone of 590 vs., the second partial of 295 vs. The two lengths thus found were 18.1 and 14.3 cm. The distance between them was then divided into five approximately equal parts (equal to the nearest integer). The four values thus obtained, together with the two extreme values experimentally determined, constituted the lengths at which the six interference-tubes for this partial were set. A similar procedure was followed for the fundamental in the other two regions, and for the third, fourth and fifth partials. All the sixth partials were also eliminated, since their frequencies were odd multiples of the frequencies of the second partials.

Some difficulty was found in keeping constant, throughout any one of these ranges, the intensity of the tone heard by the observers. There are two principal causes of variation. In the first place, the intensity of the tone produced by the variator may not remain constant. We were able to eliminate this source of error, over the relatively small ranges used, by careful adjustment of the air-pressure and the position of the mouth-pieces of the bottles. A second possible cause of differences in intensity lies in the existence of reflected waves, either in the apparatus-room or in the conducting tube, which would intensify certain frequencies and weaken others. Standing waves in the same room with the apparatus were eliminated by curtains hung on the walls of the room, and by the placing of the variators as far from the surrounding walls as conditions would permit. Reflection inside the tube was not stopped; but the conducting tube was made of such a length that reflection did not affect intensity within our range of frequencies. This length (between the last interference-tube and the rubber stopper) was determined empirically.

The sounding of the stimuli was automatic. The supply of air to the variators was controlled by a pinch-cock operated by a solenoid;

[33] These measurements are from center to center of the interference-tubes.

[34] W. Köhler, Akustische Untersuchungen, II, *Zeit. f. Psych.*, 58, 1910, 114 ff.

[35] The zero-point for these measurements is the point at which the flat piston-head of the interference-tube is flush with an element of the inner surface of the conducting tube.

it was thus possible to regulate electrically the turning on and off of the air. This was done by a rotating commutator, which also made connections for the 'ready' signals, single strokes of a muffled bell. The commutator made three revolutions in a minute. The 'ready' and 'now' signals were separated by 1.2 sec. The first tone came 1 sec. after the 'now' signal, and lasted 2.4 sec. After a pause of 2.0 sec., the second tone followed, also with a duration of 2.4 sec.[36] The complete cycle occupied 9 sec., and the observer was allowed 11 sec. to record his judgment before the signal for the next pair of stimuli.

The observers all gave written reports. Every observer was furnished with a board-clip containing a mimeographed sheet with space for 90 reports, and blank paper for introspective records. He was also furnished with typewritten instructions.

Our general procedure was a differentiation of the attributes on the basis of their difference-limens. The plan was to put the observer under instructions to judge now one, now another, of the characteristics postulated as attributes, and to determine in every case the least noticeable difference. We hoped by this method to discover, first, whether the various characteristics of pure tones could be admitted to the attributive classification on the basis of independent variability, and secondly, whether the judgments of these characteristics were made upon an attributive or an associative foundation.

The method of constant stimulus differences was used to determine the limen. The instructions, and the pitches employed as stimuli, will be stated under the various attributes. Five comparison stimuli were used in every case, objective equality and two steps above and below. Fifty series (in addition to the preliminary series) were taken for every determination, except with the observers Da and W, who completed thirty-three series only. The stimulus-pairs were given half in each time-order, and the results for the two time-orders were computed together. The order of the stimuli, as well as of the time-orders, was determined by chance; but the same order was followed for all determinations. The limens were computed from the crude data by the *phi-gamma* hypothesis, Urban's tables being used and the computations checked. Six values were found for every determination, three for the upper limen and three for the lower limen. These were the limen proper (L), the measure of precision (h), and the value $c = h \times L$. This latter value, since it is independent of the units employed, forms a measure of relative precision, comparable to the 'coefficient of variability' of statistical work. L and h are, of course, in terms of vibrations. In addition, the mean difference-limen (half the 'interval of uncertainty') was computed for every determination, as average of the upper and lower limens (L); as well as the relative difference-limen, which was obtained by dividing the mean difference-limen by the standard stimulus.

III. Vocality

Preliminary Training. Most of our observers had never made judgments of vocality before this experiment was undertaken. It was therefore necessary to train them in vocality,

[36] These times are not exact. They are the times during which the electric current operating the solenoid was flowing. There was undoubtedly some lag in the starting of the variators after the opening of the pinch-cock.

to teach them what they should look for when they observed the vowel-qualities. The training was accomplished by requiring the observers to give absolute judgments of the vocality of single tones,— much as Köhler did in his early experiments. For this preliminary work, variators were used without the interference-apparatus. The observer sat in the same room with the experimenter, a few feet away and with his back to the variators, and gave his reports orally. A series of 35 tones was made up, ranging by half-tones from 150 to 1,200 vs. During the first few hours of work with every observer, these tones followed one another in ascending or descending serial order; toward the end of the practice a haphazard order was used. Two, three, and occasionally four series were run in the course of an hour's work.

The first set of instructions was as follows:

"You will hear a relatively simple tone. Report any resemblance to any vowel or vowels (or semi-vowels), and roughly the degree of that resemblance. This is your main task: but if anything else that is relevant catches your attention, report it also. A tone will be repeated if necessary.

"The successive tones will run in order up or down the scale, as announced by the experimenter."

We found that under these instructions the observers tried to fit different vowel-sounds to a tone, and to determine which vowel was most appropriate, rather than to listen for a characteristic of the tone. After a few days, the following instructions were therefore substituted for the original formula:

"You will hear a relatively simple tone. Regard it as singing or speaking a vowel (or vowel-like consonant) to you. Report what vowel or vowels you thus hear. This is your main task: but if anything else that is relevant catches your attention, report it also."

The practice was continued until the observer was able to recognize and name the vowel-qualities of the tones with a fair degree of consistency. The time required to reach this stage varied from observer to observer.

One observer, W, was not given any preliminary practice. He had been a subject in the experiment of Modell and Rich, and was able to give vowel-judgments very readily.

Two other observers had had some practice with vowel-qualities, and gave consistent judgments from the first. Di had set up apparatus for the demonstration of vocalities in Dr. Titchener's lecture-course often enough to be familiar with this character, and to have no difficulty in hearing the vowel-qualities in the tones. He found the pure O and U lower than we had expected from previous investigations, but remained consistent in his reports through some four hours' practice. He says in his reports that "the vowels are very self-evident." C, on the other hand, was acquainted with vowels from the

practical rather than the theoretical point of view. Although he had little psychological experience, he had been trained in singing vowels, and was thus able to give judgments that were consistent; they were also in accordance with the findings of Köhler.

Y was also able to make fairly consistent judgments from the first, so that three hours of practice were sufficient in his case. His success in this form of observation was due not so much to his previous training as to the manner in which he went about the task set him. He reports: "I retain the tone, and then catch myself trying to shape my lips and throat so as to form a vowel. Sometimes I hit upon it; sometimes I am very doubtful. When I think of vocality, I think of a spoken vowel, E, I, etc., a voiceness." He had thus followed the instructions implicitly, and had come immediately to the correct method of observing vocality.

Three observers, Da, G, and O evidently relied to some extent upon their theoretical knowledge of vowels. They knew the order in which the vowels should come, in an ascending or descending series; and having given the first judgment, they would report successive vowels less than an octave apart, sometimes only a few notes apart. G, for example, would start with a judgment of M (in an ascending series), and report successively the vowels to I in a little over an octave. Her difficulty is explained in the following report: "Vocality does not 'mean' to me. I judge the vowel spatially by pitch . . . I keep unintentionally falling back on what I know from the lectures." It was necessary to demonstrate to her, through speaking and singing the vowels,[37] what is meant by vowel-quality, before her report became even moderately consistent. O also seems to have made use of his theoretical knowledge; but as he is musically trained, he judged the octave-relations correctly in the majority of cases, so that his reports were on their face consistent from about the second hour of practice on. Da started to judge in about the same manner as G, but corrected himself when shown by the experimenter what he was doing.

F and L, the two observers whose results are utilized only in the preliminary work, at first gave scattering judgments; they reported practically every vowel in every part of the scale. Neither had any notion of vocality; and even after some hours' practice it was possible to get consistent judgments only after the vowels had been demonstrated afresh by singing and speaking.

Method of Paired Comparisons. Our next step was to determine the pure vowels, the turning-points of the system of vocalities. Köhler[38], it will be remembered, had in his experiment determined these points by a limiting procedure. He started with the tone on the one side of a given vowel, and varied it by small steps until the observer first noted a trace of the next vowel beyond. We desired, however, to avoid the errors of expectation which are inherent in such a method, and to make it as difficult as possible for our observers to judge some particular tonality as representing the pure vowel

[37] Professor Weld was kind enough to assist us in demonstrating the vowels to the observers.

[38] W. Köhler, Akustische Untersuchungen, II, *Zeit. f. Psych.*, 58 1910, 111 ff.

in each octave. In order to overcome this possible tendency (to call a certain tonality the pure vowel), we used the method of paired comparisons with a haphazard arrangement of the pairs. The observers were instructed as follows:

"After two bells as 'ready' and 'now' signals, you will hear two tones near U in vocality. Report whether the second tone is nearer to U or farther from U than the first.

"Record your reports in order upon the sheet provided, indicating a report of 'nearer' by + and a report of 'farther' by —.

"Any further observations you may care to make should be written out at the end of the series on the blank paper provided."

These instructions were varied for the other vowels by inserting O and A in place of U.

The stimuli were pure tones obtained through the interference-tubes in the manner described. In the regular series there were 90 pairs. These gave all the possible combinations of ten tones in both time-orders. The stimuli were as follows: for U, 240 to 280 vs. by 5 vs. steps; for O, 480 to 570 vs. by 10 vs. steps; and for A, 960 to 1,140 vs. by 20 vs. steps. The order in which the pairs were given was arranged by lot, and was the same for the three regions. In making up these series, we were guided by Köhler's figures. The stimuli cluster about the values obtained by him for the pure vowels, and spread over as large a range as the interference-tubes would permit. With a view to equalizing the effects of practice, parts of each one of the three series were given in every observation hour. These parts varied in length from 30 to 40 pairs. With observers Da and W, however, halves of each of two series were given in every hour. Every observer had two hours of preliminary practice, with short series of 21 pairs, to accustom him to receiving the stimuli through the ear-tubes and to making written reports.

The results of the experiments by this method are the frequencies with which the different stimuli were judged as 'nearer' to the pure vowels. These can be treated mathematically by averaging the stimuli, multiplying every one by the number of times it is judged as 'nearer' (judgments of = being counted as a frequency of one-half for each stimulus). The values so obtained, together with their mean variations, are shown in Table III. It would seem from these figures that an approximate octave-relation obtains between the pure vowels: an approximation which is very close if we average the values (and also the *mv*) for the individual observers. But the averages obtained by experimentation also approximate equally well the averages that may be obtained by applying the laws of chance to these stimuli.

If, as Köhler holds, the turning-points for vocality are sharply defined, we should expect that a curve showing the frequency with which every stimulus is judged as 'nearer'

TABLE III

POSITIONS OF THE PURE VOWELS BY PAIRED COMPARISONS

Vowel		Observer							
		C	Da	Di	F	G	L	W	Y
U	Av.	261	261	258	259	270	269	256	267
	Mv.	11.2	9.5	10.7	11.1	7.8	11.6	8.6	12.1
O	Av.	533	512	520	522	519	518	529	540
	Mv.	22.3	24.1	22.9	24.9	20.9	22.3	22.7	18.8
A	Av.	1084	1085	1077	1043	1022	1059	1085	1069
	Mv.	38.2	36.4	39.6	47.9	36.3	47.3	36.4	36.5

	U	O	A
Average of 8 observers	262.5	524.0	1065.5
Average *Mv*	10.3	22.4	39.8
Octave-relation	262.5	525	1050
Mv	10.3	20.6	41.2
Average by mere chance	262.5	525	1050
Mv. by mere chance	12.5	25	50

would come to a sharp peak at the pure vowel, and fall away regularly on either side. The curves did not, however, behave in this way. Eleven of the 24 curves obtained showed two distinct peaks. We might remark, parenthetically, that these curves are not curves of any mathematical functions, since every stimulus is compared, not with the same set of stimuli, but with a different set of stimuli. It seems possible that these double predominances may be due to a shift in the attitude of the observer from day to day. We therefore considered separately the two halves of the results obtained from Da and W for the vowel O: results which, it is to be remembered, were obtained at two sittings. The fractionation showed that for Da the point of maximal judgments shifted from 500 vs. on the first day to 540 vs. on the second day, while for W the shift was from 560 vs. to 510 vs. Since these changes were in opposite directions, they cannot be due to any unnoticed variation in the stimuli, but clearly show a

change in the attitude adopted by the observer in judging a pure vowel. The other cases are doubtless similar.

In 8 of the 13 curves which did not exhibit bimodality, the frequency of judgments of 'nearer' increases steadily toward either the upper or the lower end of the series of tones with which we worked. This fact would imply that the purest vowel lay either at or beyond the end of our series. Yet the averages for such a set of judgments are still well inside our range. This result was to be expected. It proceeds from the fact that the stimuli throughout the middle of our range are naturally preferred to tones still further from the pure vowel than they are themselves. These preferences, of course, operate to pull the average away from the point of purest vocality and toward the center of the region in which we are working.

Our mathematical averages, therefore, are of little value. The close octave-relation between the averages of eight observers for the three vowels is a result, not necessarily of the positions of the pure vowels, but more directly of the octave-relations existing between the stimuli used. A very similar result would have been obtained had the judgments been scattered among the pairs by mere chance, as is shown at the bottom of Table III. We were getting out of the experiment only so much of an octave-relationship as we had previously put into it.

On the other hand, the very variability of the results is significant. We were not using a serial procedure in which the observers could stop when they came to a certain tonality. It would have been far more difficult in our experiment for the observers to pick out such a tonality, and to call it the pure vowel, than it was under the procedure followed by Köhler. His subjects may have noted some particular change, and have continued to report that change as the turning-point; while our observers could not do this, because the stimuli varied continuously throughout the region, instead of passing the significant point in regular progression; and because this significant tone, even if it actually were one of the tones of our series, occurred in only one pair out of ten. But if the figures obtained by Köhler were in reality the results of judgments of change of vowel-quality, then there is no reason why equally regular results should not have been secured by our method. Our observers had less opportunity than Köhler's to judge anything save vocality; and the very regularity of Köhler's figures would seem, therefore, to show that they are not the results of purely vocal judgments.

Vocal Limens. We next desired to find the differential limens for vocal judgments of the pure vowels. The method of paired comparisons did not furnish us, as we had expected, with definite values for this purpose. It was therefore necessary to adopt another method of determining approximately the turning-points. A single tone, repeated several times, was presented to the observer, who was asked to report whether or not it was a certain pure vowel; and, if it was not, to give the direction in which it differed from the pure vowel. On the basis of this report, the experimenter then sounded another tone, slightly different in frequency; and this process was continued until a tone was found which the observer was willing to accept as a good vowel. A short series of liminal judgments was then run (under the instructions given below); if the observer reported differences on both sides of the pure vowel, we considered that an approximation sufficient for our purposes had been attained. The same position was not always found by a given observer on different days. A tone that was, for instance, reported on a certain day to be on the A side of O would be reported the next day on the U side. Nor was it always possible to obtain a good vowel within the limit of tones produced by our apparatus. We were unable to get tones high enough to give a good A for Da, Di, and W, or a good O for C and Y. In these cases we went as near to the pure vowel as we could.

The positions for pure vowels obtained by this method constituted the standard stimuli for the determination of the vocal limens. These standards are given in Table IV. The comparison stimuli differed by 5 vs. for U, by 7.5 vs. for O, and by 15 vs. for A. The general procedure described in an

TABLE IV

STANDARDS FOR VOCAL LIMENS

Observer	Vowel		
	U	O	A
Da, W	250	530	1145
Di, G	250	580	1120
C, Y	260	580	1150

earlier part of this paper was followed. The observers were instructed:

"After two bells as 'ready' and 'now' signals, you will hear two tones, both of which are near U. You are to report the vocality of the tones (disregarding their pitch) as same or different; if you judge different, you are to give the direction of the difference, *i.e.*, to say that the one tone is nearer to O or nearer to M than the other. Your report will then take the form: 1 O, 2 O, 1 M, 2 M, or =. Enter your reports in order upon the sheet provided.

"At the end of the series, you are to make an introspective report, upon the blank paper provided, covering the processes upon which your judgment is based."

Similar instructions were used for the other vowels. When judging tones in the neighborhood of O, the observers made their reports in terms of U or A, while tones near A were reported as towards O or E.

The numerical results are shown in the accompanying tables: V, VI and VII. An examination of these tables

TABLE V.

VOCAL LIMENS: VALUES OF *L*, *h* AND *c*

Observer	Value	STANDARD					
		U		O		A	
		Lower	Upper	Lower	Upper	Lower	Upper
C	L	6.79	6.08	11.86	8.82	18.31	16.70
	h	.114	.096	.093	.057	.057	.083
	c	.774	.586	1.099	.501	1.048	1.382
Da	L	1.80	2.23	4.74	3.52	8.59	6.43
	h	.130	.121	.127	.455	.060	.466
	c	.234	.269	.602	.346	.513	.305
Di	L	6.45	8.33	11.40	11.87	23.55	22.50
	h	.109	.299	.180	.190	.128	.102
	c	.703	2.486	2.053	2.257	3.029	2.291
G	L	2.42	2.95	4.17	4.92	12.05	9.94
	h	.262	.373	.250	.212	.064	.052
	c	.633	1.099	1.043	1.043	.774	.520
W	L	3.51	2.92	4.15	4.15	5.23	5.32
	h	.223	.456	.396	.137	.067	.496
	c	.783	1.329	1.645	.567	.351	1.645
Y	L	5.56	5.56	4.53	6.98	13.81	11.45
	h	.223	.283	.139	.137	.090	.041
	c	1.242	1.573	.628	.957	1.238	.467

shows that, without exception, the limen increases with progress up the scale. The relative difference-limen, on the other hand, shows a decrease from the vowel U to the vowel O; from that point to A the change is less certain, there being an equal number of cases of increase and decrease, so that no general statement of direction is possible.

We may next inquire what sort of judgments our observers were making, as shown by their introspective reports. State-

TABLE VI

VOCAL LIMENS: MEAN DIFFERENCE-LIMENS

Standard	Observer					
	C	Da	Di	G	W	Y
U.....	6.08	2.02	7.39	2.69	3.22	5.56
O.....	10.34	4.13	11.64	4.55	4.15	5.76
A.....	17.51	7.51	23.02	11.00	5.28	11.45

TABLE VII

VOCAL LIMENS: RELATIVE DIFFERENCE-LIMENS

Standard	Observer					
	C	Da	Di	G	W	Y
U.....	.0247	.0081	.0296	.0108	.0129	.0214
O.....	.0178	.0078	.0201	.0078	.0078	.0099
A.....	.0152	.0065	.0206	.0098	.0047	.0110

ments that vocality is introspectively a part of the tone are rare:

"There seems to me to be no doubt that the vowelness is intrinsic to the tone" (Di). "Vowels seem objective, *i.e.*, not added by me but an intrinsic part of the tone" (Di). "Vowelness seems an intrinsic part of the tones as they come. Often, perhaps usually, the two tones are distinctly different as totals, but the same vowel sounds out from the different settings" (Di). "The vocality is in the tone" (Y).

Slightly less positive are the reports which say that the tone seems to 'sing' or 'speak' the vowel.

"Listened passively; tone sang the vowel-quality" (Da). "Attended passively; tones sang the vowels. . . . The tones came as O, U'ish, or A'ish" (Da). "Each of the tones seems to say its vowel to me" (Di). "I hear the tone uttering the vowel, as vocalized" (O). "Some tones shout A and others shout U" (Y). "The tone says O or OU or OA; I record what the tone speaks" (Y).

Imagery of various types is frequently mentioned:

"The vowel quality of O lies around 70-80 series in my 'number-form.' I am vaguely aware of this 'number-form,' but do not use it in making judgments" (Da). "This morning, for the first time, I had visual imagery. O was accompanied by a rough outline; A by a black streak. But I *think* that these did not affect my judgment" (G). "In judging M, there is often an auditory image of the hum for comparison" (W). "Kinaesthesis is accompanied in many cases by an auditory image of the vowel" (Y).

More important to the judgments, however, are the almost universal reports of kinæsthesis, either imaginal or actual:

"In listening for the vowel quality, I had distinct kinaesthesis when there was much difference. I suppose the thinking of the quality into the tone is the setting of the throat and other muscles to a position that gives the vowel-quality I am attending to" (C). "I doubt if I have any kinaesthesis from the throat, but I have an image (kinaesthetic) of the sensations produced by the articulation process for the vowel I hear" (C). "Repeated the vowel 'sung' in verbal kinaesthesis so as to 'fix' the image in mind. Judgment then made with the sensation of second tone and image of the first" (Da). "Passively attended to sounds; verbal kinæsthesis of pronouncing vowel quality" (Da). "In making judgments, especially when the tones are close together, I often find myself forming the corresponding vowel in my throat. This is for the purpose of making the judgments more accurate; when the throat forms the same vowel for both, the judgment is 'equal.' This method is not invariably applied" (Di). "I judged the vocality on the basis of motor tendencies to form the vowels of the tones with my lips" (G). "Tendency to form vowels of tones given, sometimes in lips, usually in throat. At other times merely a more widespread 'set' meaning E or A" (G). "Here again I get different kinaesthesis for the pure vowel, *i.e.*, I usually judge OU or OA when I get the unstable sensation. On the other hand, E often comes with its own kinaesthetic set, and I immediately get verbal-motor E" (G) (A-series). "After the tone I form the vowel incipiently with my throat muscles, sensations of strain localized in the throat. . . . Always this throat kinaesthesis is the surrogate. It retains the U-ness of the first tone for me, and comparisons are always made in terms of it. The kinaesthesis is accompanied in many cases by an auditory image of the vowel" (Y). "A kinaesthetic complex from the set of my throat, together with an auditory image (noticed in some cases) is the basis of judgment" (Y).

For some observers, however, the kinæsthesis eventually

dropped out, and the judgments came to be made immediately, though for the majority this change did not occur:

"Judgments made to a greater degree than previously on auditory impression; they come almost immediately, sometimes without the usual kinaesthesis" (G). "Judgments more immediate than usual and based on auditory impression. Motor tendencies are less articulate than they have been, and play less part in my judgment, often not coming to clearness until after it is made" (G). "The kinaesthetic feel in my throat is frequently there, but this morning the vocality judgment was immediate. I was often ready to write the judgment before the second tone was completed" (W). "Verbal kinaesthesis played practically no part. Judgments were immediate; the one had an M or O or U quality" (W).

Our observers were not at all times sure that the judgments they gave were purely vocal and uninfluenced by other attributes, as the following reports show:

"Pitch has some influence on the judgment, I think" (C). "Several times noted pitch. Once the pitch seemed equal and the vowel-sounds O and U'ish. Another time, the pitches were different and the vowel qualities seemed equal" (Da). "A few times there was kinaesthesis only in my larynx, but in those cases I was not sure that my judgments were purely 'vocal,' *i.e.*, that pitch was not also a factor" (G). "I find a tendency to take the first tone as standard, modifying it subjectively to the utterance of the vowel. The second tone, then, because of its difference, is more apt to be judged less like the vowel. If it is higher, it becomes in this case A; if lower U. . . . The A and U qualities are not so much recognized as inferred" (O). "Judgments easier than usual. I fear that this is partly a result of recognition of the standard, and that pitch judgments cut across" (W).

We also find a number of descriptions of vocal differences in terms that are ordinarily used to describe other attributes:

"Sometimes there is a more widespread kinaesthesis which seems to have the meaning 'more open' or 'more closed'" (G). "The E is the most definite vocality; it *shrieks*" (G). "The tones I judged as like A were more open and more like A in this sense. They seemed too open for O, but they have very little A quality (whatever that is) aside from the openness" (W). "The tones I judged as E were 'harder' . . . E has this 'hard' quality as compared with the softness of a good A" (W). "Vowel is given immediately. E-ness is hard, small, cramped up, O-ness is more diffuse, large, open, and soft" (Y).

At the opposite pole to the statements that vocality is intrinsic to tones, stand the reports that an observer found it necessary to 'think' the vowels he heard into the tones:

"In judging the vowel, I seem to think the vowel into the experience" (C). "The tone does not speak the vowel to me. I rather think the quality into the tone. I expect the vocality and interpret the tone as that, within the limits of suggestion" (C). "The A and U

qualities are not so much recognized as inferred" (O). "Tried to try out on the first tone the peculiarities of O, UO, and OA, and reach a tentative judgment before the second tone" (O).

Discussion. We may now ask what evidence these facts adduce with regard to the status of vocality as an attribute. In the preliminary work it was found that only those observers who had had some sort and amount of previous training with vowels were able at once to give judgments of vocality. Of the others, several tried to make judgments on the basis of theoretical knowledge. When they did this, they were able to fit the complete vowel-series into a very short range, and to make vocal reports very different from those they learned to give later on. It was, it will be remembered, necessary to show three observers, by speaking and singing the vowels, which vocalities were to be heard at various parts of the scale. There are two possible interpretations of such teaching. We may have been pointing out to our observers what they were to look for in observing vocality; or we may have been building up perceptions of the vowels (perceptions that already existed for the observers accustomed to vocality); it was then to be expected that these perceptual judgments should, with practice, become as immediate as attributive judgments.

The outstanding results of our experiments by the method of paired comparisons were the extreme variability of the results and the lack of octave-relations between the pure vowels (save such octave-relations as were due to our choice of stimuli). The latter result is at variance with that obtained by Köhler with a limiting procedure. It echoes the suggestion made by Stumpf[39] that Köhler's observers were judging some particular tonality as the turning-point, a judgment which our observers were not so liable to make.

The quantitative figures for the vocal limens of themselves tell us little. They show a high degree of consistency between the observers with respect to the course of the limen, although the values themselves show considerable individual differences. The introspective views, however, exhibit a number of important features. Statements that the vowels seem to be inherent in the tones are balanced by reports that other observers have to 'think' them there. It would seem that the latter are the more significant, since a report of inherence might have its roots only in the observer's adaptation to a particular type of perceptual judgment.

Kinæsthesis, together with verbal and auditory imagery, was

[39] C. Stumpf, Über neuere Untersuchungen zur Tonlehre, *Ber. ü. d. 6 Kong. f. exper. Psych.*, 1914, 305 ff.

prominent in most of the observers. It always tended to drop out, and in several cases completely disappeared. The presence of such surrogate imagery, however, is only a very weak implication that the judgments are not attributive. Those observers who lacked auditory imagery were forced to adopt some other means of remembering the first tone in order to compare it with the second.

Not only did our observers occasionally find pitch influencing their vocal judgments, but they give descriptions of differences in vowel-quality in terms of other attributes, speaking of vocalities as open, closed, hard, soft, cramped up, and shrieking. These terms seem more akin to descriptions of volume or brightness than of vocality.

On the whole, it would appear that vocality has not, in these experiments, shown its right to be classed as an attribute. The judgments of vowel-quality seem rather to be judgments of perceptions, perceptions which we found ready-made in some observers, and built up in others. A long series of studies in the theory of vowels has shown that a given vowel-sound always contains a predominating tone or tones in a certain region of the scale, and that the regions are approximately an octave apart. These predominating tones form the core of the perception of vowels. If, as in our experiment, the core is presented to an observer who is instructed to hear the vowel, the remaining elements are supplied in some individual fashion, and the vocal judgment is rendered.

IV. Pitch

The experimental work on pitch consisted of a determination of the differential limen by the standard method. The instructions were as follows:

"After two bells, as 'ready' and 'now' signals, you will hear two tones. You are to report the relative pitch of the tones, judging the second in terms of the first. Your report will then take the form: "higher," "equal," or "lower." Enter your reports in order upon the sheet provided.

"At the end of the series, you are to make an introspective report, upon the blank paper provided, of the processes upon which your judgment is based."

No provision was made in the instructions for "doubtful" judgments. The instructions used in determining limens for vocality were already so complicated that it was not considered wise to add any further categories; and since we wished to make the instructions for the different attributes comparable, we must needs omit mention of "doubtful" judgments throughout all the instructions. The observers did, nevertheless, occasionally give judgments in this category (as well

as judgments of the "or" type). George[40] has shown that such judgments, when they occur, involve a shift in the attitude of the observer toward the impressions, and that they should, so far as possible, be eliminated in psychological work. We accomplished this end by discarding all "doubtful" or "or" judgments, and repeating the stimuli in a later series. We did not, of course, eliminate all judgments in which the observer was doubtful, for the instructions would tend to make him neglect to report as such judgments that were in reality of this type. Whatever bad effect our procedure may have had upon the psychometric functions was the same for all determinations.

The standard stimuli used were 275, 550, and 1,100 vs. These values were obtained by averaging the standards of the

TABLE VIII

PITCH LIMENS: VALUES OF *L*, *h*, AND

Observer	Value	Standard					
		275		550		1100	
		Lower	Upper	Lower	Upper	Lower	Upper
C	*L*	2.76	2.81	3.65	3.50	8.09	6.85
	h	.580	.681	.242	.452	.178	.259
	c	1.601	1.915	1.306	1.583	1.440	1.771
Da	*L*	.61	1.10	.84	.67	1.18	1.49
	h	.430	.468	.276	.491	.325	.255
	c	.260	.517	.231	.334	.383	.380
Di	*L*	1.65	1.57	1.92	2.22	3.19	3.80
	h	.433	.536	.319	.375	.263	.282
	c	.715	.829	.612	.796	.838	1.071
G	*L*	1.30	1.43	.82	1.08	1.80	1.89
	h	.437	.376	.206	.388	.263	.254
	c	.568	.556	.171	.419	.473	.481
O	*L*	.59	.56	1.22	.89	3.18	3.17
	h	.636	.782	.411	.639	.304	.282
	c	.378	.396	.499	.574	.967	.898
W	*L*	.40	.32	.39	.42	.49	.90
	h	.620	.741	.437	.806	.383	.331
	c	.250	.259	.171	.345	.188	.299
Y	*L*	.37	.41	.60	.49	.69	1.51
	h	.694	.969	.383	.708	.431	.366
	c	.254	.392	.230	.345	.305	.538

[40] S. S. George, Attitude in Relation to the Psychophysical Judgment, *Amer. Jour. Psych.*, 28, 1917, 1 ff.

vocality series. The distances between the comparison stimuli (determined in the preliminary work) were, for all observers except C, 1.0, 1.0, and 1.5 vs., respectively, for the three standards; for observer C they were 2.0, 2.0, and 3.0.

A number of preliminary series were run through to overcome the effects of initial practice, and to determine the size of steps in the further work. This preliminary practice amounted to three hours for Da and W, and to about six hours for the other observers.

The numerical results are given in Tables VIII, IX, and X. Consideration of these figures shows us that the *DL* for pitch always increases from 550 to 1,100 vs. Between 275 and 550 vs. there are changes in both directions, although the limen is always larger at 1,100 than at 275. The relative difference-limen decreases from 275 to 550 vs., and tends to remain constant in the next octave. Its course is thus similar to that of the relative *DL* for vocality, though the decrease is less marked. The absolute limens are in every case much smaller for pitch than for vocality. It will be noted that the limens for two observers, C and Di, are unusually high. In the case of Di, we have no reason to suspect anything anomalous about the judgments, both because of the introspective evidence and also because the limens obtained from him are habitually large. He represents a very cautious type of observer, and reports a difference only when he is thoroughly convinced of its existence: this attitude naturally results in large values for the limens. C, on the other hand, was certainly judging something other than pitch, and will be separately considered.

Reports that pitch is intrinsic to the tones were rendered as in the work on vocality, and by the same observer. There

TABLE IX

PITCH LIMENS: MEAN DIFFERENCE-LIMENS

Standard	Observer						
	C	Da	Di	G	O	W	Y
275.....	2.79	.85	1.61	1.37	.58	.36	.39
550.....	3.65	.75	2.07	.95	1.06	.41	.54
1100.....	7.47	1.34	3.50	1.85	3.17	.67	1.10

TABLE X

PITCH LIMENS: RELATIVE DIFFERENCE-LIMENS

Standard	Observer						
	C	Da	Di	G	O	W	Y
275.....	.01015	.00309	.00585	.00498	.00211	.00131	.00142
550.....	.00664	.00136	.00380	.00173	.00192	.00074	.00098
1100.....	.00680	.00122	.00327	.00168	.00288	.00061	.00100

are, however, no conflicting statements that the judgment has to be 'thought' into the experience.

"There is a difference which I am willing to call highness, and which occurs in small gradations. . . . It seems fundamental" (Di). "So far as I can find, the judgments are sensory and not perceptual. The difference is intrinsic" (Di).

The immediacy of pitch judgments is also testified to:

"Judgments easy and usually given immediately" (Da). "Pitch judgments not difficult to make, but I do not know, as yet, the basis for them. . . . All I can say is that the tones are qualitatively different; I do not know the difference" (Di). "Judgments made automatically; I try to describe afterwards" (G). "Judgment comes immediately after the sounding of the second tone" (Y). "The judgment is made immediately after the sounding of the second tone, and is made automatically. I merely write the judgment" (Y).

Kinæsthesis plays a part in the judgments of all observers:

"Judgments made in terms of kinaesthesis. . . . Kinaesthesis seemed to be sustained during the period between the stimuli, and it would have a tendency to 'let down' on the perception of the second tone. When it 'let down,' I reported tone as lower; when it 'stepped up' a step, I would report as higher" (Da). "Kinaesthesis aided to retain memory of first tone. Have no other method, apparently, of retaining first tone (to my satisfaction). Memory after-image is no good, and I cannot get a positive after-image" (Da). "There is some kinaesthesis, but it is not essential, I think" (Di). "I cannot find any kinaesthesis which does not seem to be unnecessary" (Di). "I 'hold' the first tone by a general, *i.e.*, widespread, body-set; if at the second tone this relaxes (it seems to 'drop'), I call it lower; if the tension gets higher (the strain sometimes rises higher in my throat), I say higher" (G). "Very frequently I recognize the interval as a musical interval in sol-fa, but usually a bit sharp or flat. In these cases throat kinaesthesis is present as if singing the tone" (W). "Judgments immediate; only in cases of doubt did I refer to kinaesthesis. I suspect, however, that the 'sharpness' or 'flatness' I have been reporting is at bottom kinaesthesis" (W). "During the first tone I adjust the throat muscles as if singing the tone; I hold my

breath. This kinaesthetic set retains the first tone for me until the second sounds" (Y). "I attend to the straining in my throat. This steadies the tone and is the basis of judgment" (Y).

The kinæsthesis, however, tends to drop out, so that the judgments are more immediately based on auditory imagery:

"Judgments easy and usually given immediately, in terms of 'higher' or 'lower'; at times of doubt judgments are made in terms of kinaesthesis. Kinaesthesis is not playing the important rôle it did at first" (Da). "I cannot find any kinaesthesis which does not seem to be unnecessary" (Di). "I tried to inhibit the organic strain and relaxation, and I can still make pitch judgments" (G). "As the series progresses, I come to rely less and less upon the kinaesthesis and sometimes become quite inattentive to it, and make the judgment on the basis of the quality of the auditory impression" (G). "Kinaesthesis does not form a basis for the judgment. The judgment is made on some qualitative difference" (G). "Judgments immediate. Occasionally, in cases where impression did not touch off judgment with certainty, I vocalized in internal speech" (W). "I often listen for second tone with auditory image of first tone; judgment of higher, lower, or equal is immediately touched off. . . . Sometimes there is a distinct organic or muscular accompaniment which may be a cue, but the judgment comes so quickly that I doubt whether the organics are necessary to it" (W). "Judgments come immediately. In this series, the auditory image was frequently present when the second tone sounded, and the throat kinaesthesis played less part than usual" (Y). "Auditory image of first tone is the basis of judgment. Occasionally, there is a trace of throat kinaesthesis (imagery)" (Y).

Several observers are at times in doubt as to the basis of their judgments, or are unable to give any description of pitch differences:

"All I can say is that the tones are qualitatively different. I do not know the difference" (Di). "Still fishing for adjectives. 'High' and 'low' are all right for the gross differences, but I do not like them for the small differences here" (Di). "Most that I can say positively is that the tones are different, and that they arrange themselves according to these differences. I do not know how to describe this difference" (Di). "I have not the slightest idea what pitch is" (G). "I find it very difficult to say what my criterion is. When the difference is considerable, the judgment is immediate" (W).

Sometimes the pitch-difference is described in terms which we should expect to find used for brightness rather than for pitch:

"The attribute seemed to differ along a 'far-near' continuum" (G). "High tones are clearer and brighter. They seem harder, too" (G). "'High' tones are sharper; 'low' are more diffuse" (G). "Higher tones seem more closed, perhaps; lower tones more open" (G). "The lower tones are duller, the higher are brighter; the lower are blunter, the higher are sharper. I do not know, when I judge pitch, whether these so-called qualities are the basis for my judgment" (W).

Observer C, it will be remembered, gave a pitch-limen markedly different from that of the other observers. His introspections show unmistakeably that he was judging something other than pitch:

"Difference in pitch appears to be in relation to the key base created by the first tone." "I do not get that higher or lower quality that generally characterizes pitch. I just get a hint of it in the tones that are far apart relative to the series. When they are near together, I get the 'out of tune' business." "I still hear the tones as dependent upon some key." "Today I did not get a discord effect with change in pitch. At least I did more than say the criterion of difference in pitch was *discord*. This occurred on two occasions. When I particularly paid attention to the last tone, and when I tried to look for the cause of the difference, my attention immediately shifted to my throat. The effect was like that obtained in reaching for a high note in one's register. . . . It seems to me that this discord is relative to the first tone of the presentation."

Another observer, O, gave similar reports of musical characteristics:

"Judgments made on basis of musical steps . . . a definite tonality often apparent, though unnamed." "At the lower pitches there seem to be two tones, individuals in a musical sense perhaps. At the higher pitches, there may seem to be one tone varying in its purity." "Tonality established near beginning of series and seemed to persist throughout. That is, initial tone was of the tonality; others were 'off.'" "Tonality played a prominent part. . . . The second tone was a different tone, higher or lower, or mistuning of the first, or a trend in one direction or the other." "Judgments on the whole easy and assured. Usually on the basis of tonality, that is, difference of a semitone or in that direction."

It would seem, then, that these two observers were not giving purely pitch-judgments, but were concerned with some musical feature of the tones. O felt that he was often judging tonality, and it is quite possible that C was doing the same sort of thing. We must, therefore, disregard the numerical results obtained from these two observers, so far as our study of pitch is concerned,—keeping them in mind, however, as possible limens of tonality. W also gave a few reports of 'flatness' and 'sharpness,' but he was able to find a basis in kinæsthesis for these two terms, so that there is no warrant for believing that he was giving other than pure pitch-judgments.

Pitch appears to have justified its place among the tonal attributes. Several observers declared that judgments of pitch were immediate and fundamental, that the attribute was intrinsic to the tones. Similar reports were, to be sure, made about vocality. But in the case of the latter characteristic there were opposing statements, to the effect that it had to

be 'put into' the experience by the observer, while no such statements were made concerning pitch. Where the judgments were not immediate, kinæsthesis was used in forming the judgment. The presence of such surrogate imagery need not be a bar to the view that the judgments were made on an attributive basis. Some observers lack auditory imagery, or can use it only with difficulty, so that they must necessarily use kinæsthesis in order to hold the first tone in memory long enough to compare it with the second tone. Moveover, our observers found that the kinæsthetic accompaniments of their judgments tended to drop out.

It is noteworthy that pitch was not described in its own terms, and indeed seemed difficult to describe. When description was forthcoming, it was in terms of brightness. We must, therefore, leave open the status of pitch as an *independent* attribute until we have considered the work on brightness.

V. Volume

The attribute of volume has already been investigated by the methods that are used in this study[41]. These earlier experiments showed that judgments of volume are made upon an attributive basis, and that the *DL* for volume is approximately proportional to the stimulus. If the above results, obtained without the use of interference-tubes, could be verified with pure tones, it would be possible to take them over bodily for our purposes.

Taking as a basis the volume-limens that had already been determined, we made up series for volume with steps of 6, 12, and 25 vs. for the standards 275, 550, and 1,150 vs.[42], respectively. The steps were thus proportional to the standard stimuli. The observers were instructed as follows:

"After two bells, as 'ready' and 'now' signals, you will hear two tones. You are to report the relative size or volume of the tones (disregarding their pitch), judging the second in terms of the first. Your report will then take the form: "larger" ("greater"), "same" ("equal"), or "smaller" ("less") Enter your reports in order upon the sheet provided.

"At the end of the series, you are to make an introspective report, upon the blank paper provided, of the processes upon which your judgment is based."

Eighteen series were obtained from each one of the six

[41] G. J. Rich, A Preliminary Study of Tonal Volume, *Jour. Exper. Psych.*, 1, 1916, 13 ff.

[42] It was found that with the large differences employed it was not possible to keep the intensity of tones centering about 1100 vs. constant. A shift of the standard to 1150 vs. removed this difficulty.

observers. Inasmuch as, for five of the six, the results were nearly the same as those previously worked out with impure tones, the experimentation was not continued further with these five observers. Not sufficient data were secured for formal computation of the results; but, by plotting the frequencies graphically, it was possible to arrive by inspection at the approximate limens shown in Table XI.

TABLE XI

APPROXIMATE VOLUME-LIMENS (18 series)

Standard	Observers				
	Da	Di	G	W	Y
275	5	6	7	5	4
550	13	12	13	9	10
1150	24	30	25	22	24

It must be remembered that these limens are the merest approximation made from scanty data, and that the observations upon which they are based were performed without any preliminary practice. The figures do, nevertheless, approach very closely to a doubling of the limen for each octave, verifying the results obtained without interferences. The absolute values, however, are slightly smaller than the limens found in the earlier experiment; this discrepancy, which may be accounted for by the difference between pure and slightly impure tones, is here of no importance.

Turning now to the introspections, we find that reports of kinæsthesis as a basis of judgment occurred, although they were much rarer than under either of the previous instructions:

"The basis is usually kinaesthetic and tactual: 'large' notes make my mouth muscles contract. I get tactual imagery in my mouth, too. The 'small' tones are more sharply localized; the 'large' tones seem to have a setting, definite and deep" (C). "Occasionally I get kinaesthesis" (G). "There is strain and relaxation kinaesthesis in the throat" (Y). "A large tone is one that causes relaxation of chest muscles" (Y). "At times, judgments are made without kinaesthesis, I think" (Y).

As was the case with pitch, our observers found considerable difficulty in describing the basis of volume-judgments:

"I have not the least idea of the basis of judgment. It is not pitch, for I sometimes perceive a difference of pitch and still report 'equal.' I think it is empathy" (Da). "I do not think empathy is the basis as I did the last time; in fact, I am sure it is not" (Da). "Do not know what my criteria are. Do not really know what a difference in volume is" (Da). "I should like to know what I am supposed to judge. Volume is an indefinite something. I am fishing around to find something that I can reasonably call volume" (Di). "I do not know what or how I am judging" (Di). "Cannot describe the difference accurately yet. Judgments come fairly easily, but I cannot determine what my criteria are" (G). "I do not know what basis of judgment is" (Y). "Largeness seems to be something in the tone, although I cannot say what" (Y).

Descriptions of volumic differences, when given, are in terms of volume itself, rather than of some other attribute:

"Judge in terms of size of resonance in my ears. Sometimes it is deep or full; at other times it is high and thin" (Da). "Large tones are heavier, more spread out, and seem more unified than the smaller" (G). "The small tones are thin. The large tones are diffuse, and seem to have more texture" (G). "I find the judgments fairly easy. The larger tones are more roomy or diffuse; the smaller are tighter or more compact" (W). "The diffuseness and compactness of the tones were my criteria" (W). "It seems as if one tone is more thread-like than another. The other tone is blunter and thicker" (Y). "Apart from vocality and pitch, the tone seems cramped up, pinched together, if small; and open, spread out, if large" (Y). "The judgment is on the basis of something in the tone itself. The large tones are round, resonant, and the small tones are thin, weak, more flimsy. The judgments are easier now than before; they are made immediately" (Y).

O gave results that were markedly different from those of the other observers. We therefore made a special determination of his volume-limen by the regular method (33 series), after seven hours of preliminary work. The steps between stimuli were 1, 2, and 5 vs., for the standards 275, 550, and 1,100 vs., respectively. The quantitative data are given in Table XII. The introspections were as follows:

"The judgments were not all of equal merit. It proved easy to abstract from pitch, but the impressions of 'volume' that I got in the pitch series did not recur in the same way. It appeared as if they wanted the 'pointing' the pitch attribute gave them. Lacking this, there was difficulty in comparing the 'volumic outlines' of the successive tones." "The volume seems to be in the head, a kind of spread-outness of the tone; and in the deeper ones reverberation was noted." "There is an apparent difficulty in that some tones are ragged about the edges; wave-like fluctuations seem to surround the nuclear tone. I was aiming at a judgment of the tone itself, because these fringes seemed adventitious." "I have not yet satisfied myself as to the criteria of difference in volume. The most important factor in this series seemed to be a kind of resonance. Some tones were remarkably full, and seemed to be localized in sensation about the eardrums, and in the forehead between the eyes." "The volume-attri-

bute is elusive. I am not sure that I distinguished it clearly from pitch in this series." "The method of comparison seems to be the holding of the pattern of the first until the second occurs, and then by an overlapping of the two the impression is immediate." "These judgments are made directly on the basis of resonance. The tones seem to spread over the face from both ears to the nose, and the difference of feel constituted the difference judged."

TABLE XII

VOLUME-LIMENS FOR OBSERVER O

Value	Standard					
	275		550		1100	
	Lower	Upper	Lower	Upper	Lower	Upper
L	2.0	1.65	1.13	1.03	4.56	4.35
h	2.71	.507	.364	.345	.109	.121
c	.555	.835	.413	.353	.497	.527
Mean *DL*	1.85		1.08		4 45	
Relative *DL*	.00337		.00193		.00405	

It seems clear that O approached the task of judging volume-differences in an attitude different from that taken by the other observers. Indeed, we may doubt if his attitude was constant enough, or his judgments homogeneous enough, for the figures obtained from him to be considered limens of any single attribute. The kinæsthetic reaction (which, for the other observers, is least marked in the case of volume) suggests a possibility of pitch or brightness judgments. It is also worthy of note that, shortly before O took part in these experiments, he had read Watt's latest work on tonal attributes[48], and had been impressed by the theories there set forth. He had in mind the idea of tonal volumes as represented in Watt's diagrams; and he spoke in his reports of "volumic outlines," and of "wave-like fluctuations" that surround the "nuclear tone." We cannot, therefore, accept the figures obtained from O as volume-limens in the sense in which the term 'volume' is used by the other observers.

The majority of the observers found that volume-judgments are given directly and immediately. The kinæsthesis always at work in the judgments of some observers was indeed

[48] H. J. Watt, *The Psychology of Sound*, 1917.

present, but in a less degree than in any other type of report we have thus far considered. So immediate were some of the judgments that the observers could not even say 'what' they were judging. They could not find terms in which to characterize their criteria; the volume simply was there, and was judged. Yet the limens obtained were, even in a short series, nearly alike for all observers. And when the criteria are finally described, the terms used (full, thin, sharply localized, diffuse, spread out, compact, round, flimsy) refer directly to volume, without involvement of any other attribute.

We may accept, then, the earlier conclusions, that judgments of volume are made upon an attributive basis, and that the volume-limen tends to follow Weber's Law, as equally valid for pure and for slightly impure tones. In short, volume must be admitted to our list of attributes on the grounds both of inseparability and of independent variability.

VI. Brightness

Brightness was attacked by the same method as the other purporting attributes. The instructions were as follows:

"After two bells, as 'ready' and 'now' signals, you will hear two tones. You are to report the relative brightness of the tones, judging the second in terms of the first. Your report will then take the form: "brighter" ("lighter"), "same" ("equal"), or "duller" ("darker"). Enter your reports in order upon the sheet provided.

"At the end of the series, you are to make an introspective report, upon the blank paper provided, of the processes upon which your judgment is based."

The stimuli differed by steps, for Da and G, of 2.0, 2.0, and 3.0 vs., and for W, of 1.0, 1.0, and 1.5 vs., for the three standards, 275, 550, and 1,100 vs., respectively. The size of the steps was determined by two or three hours of practice which preceded the regular series.

The numerical results for four[44] observers are shown in the accompanying tables: XIII, XIV, and XV. As the observers fall into several distinct groups with regard to the basis of their judgments, we can best consider these data in connection with the introspections.

Di found himself unable to make brightness judgments. He writes of his difficulty:

"Brightness has two meanings for me. (1) Used to describe visual sensations, it means the qualitative difference which I perceive between

[44] Unfortunately, all data obtained from *O* have been lost, with the exception of the figures for the mean *DL*, and certain introspective statements which he includes in a published paper to which we refer below.

greys. (2) It may also be used to describe a relatively simple affectively-toned perceptual pattern. In this use, the mode of the process does not seem to matter: bright picture, bright face, bright music, bright talk, etc. I might use the word in this last sense, but I take the instructions to imply an attribute of the tone. This is where I am at a loss. Introspectively, I do not find anything that cannot be described as vowel or pitch."

Two observers, G and W, definitely identified pitch and brightness. They found, indeed, that kinæsthesis came in as surrogate imagery; but it appears to have played only the same part, and to drop out in the same way, as in other types of judgment:

TABLE XIII

BRIGHTNESS-LIMENS: VALUES FOR *L*, *h*, AND *c*

Observer	Value	Standard					
		275		550		1100	
		Lower	Upper	Lower	Upper	Lower	Upper
Da	*L*	2.78	2.18	2.86	3.40	3.72	4.02
	h	.212	.289	.204	.429	.269	.154
	c	.592	.629	.604	.727	1.000	.619
G	*L*	.82	.97	1.27	.83	1.55	1.43
	h	.408	.420	.262	.248	.224	.277
	c	.336	.407	.343	.412	.348	.398
W	*L*	.34	.49	.43	.22	.82	.66
	h	.672	.314	.521	.561	.448	.427
	c	.226	.639	.227	.123	.367	.270

TABLE XIV

BRIGHTNESS-LIMENS: MEAN DIFFERENCE-LIMENS

Standard	Observer			
	Da	G	O	W
275	2.48	.90	.79	.42
550	3.13	1.05	.69	.33
1100	3.87	1.49	2.06	.74

TABLE XV

BRIGHTNESS-LIMENS: RELATIVE DIFFERENCE-LIMENS

Standard	Observer			
	Da	G	O	W
275	.00901	.00327	.00290	.00153
550	.00569	.00191	.00126	.00060
1100	.00352	.00135	.00187	.00067

"I feel that I am making judgments as I was making them at the end of the pitch-series, *i.e.*, the pointed, focused tones are brighter, as they are higher. Here, however, the visual imagery which came with my old notion of pitch does not cut across, and the judgments are easier, simpler things" (G). "These judgments are the easiest I have yet made" (G). "I really think that, if I had not been brought up on pitch, . . . pitch-judgments would have been made from the first in terms of pointedness or brightness instead of being cut across, as they were, by visual schema and kinaesthetic processes" (G). "The criteria are the same as those in my final pitch-judgments. There is occasional kinaesthesis; it is not clear or defined in this series, and I am not sure that it is relevant" (G). "Kinaesthesis with some tones, but the judgments brighter and duller are based, as before, on something intrinsic in the tones" (G). "There is no doubt in my mind that pitch and brightness, when the former is stripped of its nasal and kinaesthetic contexts, are the same thing" (G). "Find it a bit difficult, at first, to avoid pitch-judgments. As the series progresses, the judgments become more immediate. . . . I occasionally get slight sense-feeling components, depression for duller and excitement for brighter" (W). "I am getting away from higher-lower. There were a number of times when I gave brightness-dullness judgments without any knowledge that the tone was higher or lower. The sense-feeling thing still persists, however" (W). "Judgments more immediate. . . . The sense-feeling is usually there, though I do not always observe it. The tone itself seems to be brighter, darker, or duller; and also sharper or blunter" (W). "It seemed to me several times that the difference I have heretofore called pitch is brightness or dullness, *i.e.*, several times when I made the judgment it seemed to me that there was nothing more to say about the tone" (W). "Judgments immediate. Some tones are brighter . . . but brighter may be sharper. Other tones are darker, duller, blunter, flatter, all of which mean the same thing" (W). "It seems to me, now that this series has come to a close, that differences in pitch are differences in brightness-dullness. In the pitch-series there was always a qualitative something which, at the time, seemed brighter-duller; but I was afraid to set myself for brightness-dullness for fear I should spoil the pitch-series. In the brightness-series, time and again, the brightness-judgment came immediately and was followed by a recognition of the pitch-difference (the tone was higher or lower), but I could find no qualitative difference between the two tones other than the brightness

or dullness" (W, in statement written after the last series under these instructions).

The limens obtained for pitch and brightness with these two observers are shown in Table XVI for purposes of comparison. While they are not exactly the same, it will be seen that the correspondence is very close, especially in the case of W.

TABLE XVI

COMPARISON OF MEAN DIFFERENCE-LIMENS

Standard	Observer			
	G		W	
	Pitch	Brightness	Pitch	Brightness
275	1.37	.90	.36	.42
550	.95	1.05	.41	.33
1100	1.85	1.49	.67	.74

The observers came to the judgments of pitch and of brightness in a different attitude, due to the different instructions. This fact alone would be sufficient to cause a difference in the resulting limens, even though they were limens of the same attribute; for it has been pointed out by George[45] that any shift in attitude may have an effect upon the psychometric function. Moreover the introspections bear witness to the change of attitude. W stated that, in the pitch-series, he was afraid to let himself notice brightness and dullness for fear of spoiling the series. And G, with whom the difference is more marked, reported that visual and kinæsthetic processes were constantly cutting across her pitch-judgments, but bothered her little under the instructions for brightness. Here, very plainly, is a change in attitude.

While introspective reports from O are lacking, we have his statements made when he is discussing the attributes of sound some time after the conclusion of this experiment.[46]

"In listening to a pure tone I was impressed by the pitch predominance of which Watt speaks, and also by the surrounding aura of volume. I also felt that I could detect differences in *emergence* of the pitch salient from its surrounding volume. This latter effect I designate as *brightness*." "Brightness is the emergence of the salient from the volumic mass."

[45] *Op. cit.*

[46] R. M. Ogden, The Attributes of Sound, *Psych. Rev.*, 25, 1918, 232.

This was, of course, a new attitude toward judgments of brightness, and was obviously a result of the observer's concept of tones according to the schema proposed by Watt. There is, however, a point of contact between this description of brightness-differences and the descriptions given by G and W, in that the latter two observers spoke of brightness as 'pointedness' and dullness as 'flatness.' A tentative identification of the brightness-judgments of O with those of the other observers can be made on this basis, especially since the numerical results for O do not differ radically from the figures obtained from G and W. Comparisons cannot, of course, be made with O's pitch-limens, since he seems to have given judgments not of true pitch, but of some musical quality.

The limens obtained for Da are much higher than those for any other observer under the same instructions. His introspections make it evident that his judgments were also of a different nature:

"Judgments of whatever I am making come very easily and rapidly. I think 'dull' means flat; at least I have so interpreted it in this series. 'Bright' means active, sharper. I try to abstract from pitch,—but with these concepts and this interpretation of your directions I do not see how I can." "Tried to inhibit kinaesthesis so as to keep from making my judgments in terms of pitch." "My criterion for judgment, I think, is pitch. I do not try to inhibit throat kinaesthesis, but I do try to attend away from it to the qualities of dullness and brightness. I do not know what these qualities are, but I set myself for them." "Very successful in attending away from pitch; could not have told pitch in half-a-dozen tones."

Da could not be expected to identify brightness with pitch, because he definitely interpreted the instructions as meaning that he should judge something other than pitch. Hence his concern as to whether he was attending away from pitch or not. If the two observers who gave themselves up to the instructions, and found brightness to be identical with pitch, were giving true judgments of brightness, it is clear that Da was not doing so. He was, rather, reporting the next difference he could find beyond pitch, whatever it might be, and calling it brightness.

So far as observers G, O, and W are concerned, brightness seems to have established its place as an attribute. We cannot, however, consider it a separate attribute, but must identify it with pitch, for a number of reasons. In the first place, two observers themselves specifically identify these characteristics. Moreover, the majority of descriptions of pitch were couched in terms of brightness, and when the observers turned their attention to this latter attribute they found the judgments to be both easy and fundamental. One observer, indeed,

was unable to give brightness-judgments. It is noteworthy, however, that this same observer did not, at any time, characterize pitch in terms of brightness. The new instructions, then, could not mean pitch to him, and there was nothing left upon which he could report. In addition, the limens for pitch and brightness differ only so much as might be expected from the different attitudes set up by the instructions.

We cannot say that either the brightness or the pitch concept of this joint attribute is fundamental. Although brightness may be the easier judgment for one observer, it is a meaningless term for another. A middle course may, therefore, be steered with regard to nomenclature by using the designation pitch-brightness.

VII. Tonality

The most usual description of the purporting attribute for which we are using the term *tonality* is that it recurs in every octave, so that two notes lying exactly an octave apart have the same tonality but different pitch. This description, however, does not lead to a treatment of the characteristic by our regular method. If we were to ask our observers to judge octave-similiarity, we must needs present to them, for purposes of comparison, a tone differing from the standard by about an octave. The necessary multiplication of stimuli, from two to three or four, would put the resulting judgments upon a very different basis from that provided for the other tonal characteristics, and would make them incomparable with the others. Moveover, reports of the relations of two tones to some third tone in another octave would tend to become judgments of interval rather than of the tones themselves. Finally, it would be physically impossible with our apparatus to present in rapid succession two tones an octave apart, and at the same time eliminate the second partial of the lower tone. It was therefore necessary to find another mode of attack.

It has been suggested[47] that, inasmuch as it is possible for an observer to say that two tones are different when the separation is so slight that he cannot tell the direction of difference, the report of mere difference is a judgment of tonality and the report of direction a judgment of pitch, so that the differential limen is lower for tonality than for pitch. Acting upon this suggestion, we ran a series with W in which he was asked to judge merely whether the two tones were the same or different. On the numerical side, we could not compute

[47] Révész, *op. cit.*

any limens, since the procedure under the *phi-gamma* hypothesis requires that judgments other than 'equal' be classified on the one or the other side of equality. On the introspective side, the experiment was equally negative. W found that every difference which he reported could be definitely classified as a difference in brightness. There was, then, no reason to believe that tonality was involved.

Tonality has also been defined as that attribute by which (musical) tones are named[48]. If, then, we could have our observers judge whether or not two stimuli gave the same 'tone,' in a musical sense, without regard to any other characteristics, we should have judgments of tonality under this definition. At his own suggestion, W set himself the task of disregarding brightness and of judging only whether the two tones were or were not the same, and a determination of the *DL* (25 series) was made under this 'Aufgabe.' The mean difference-limens so obtained are shown in the last column of Table XVII[49]. In his introspective reports, W was confident that his judgments were of a type different from anything he had heretofore experienced in this experiment. Not only was he certain that brightness did not enter into the judgments, but in addition he was often able to state whether stimuli which he reported as 'the same tone' were equal or different in brightness; so that reports of the type "same, brighter," or "same, duller" were frequent. It is evident, therefore, that we have here limens of musical individuality (W is a trained musician); and, according to one definition, limens of tonality.

TABLE XVII.

TONALITY-LIMENS

Standard	Observers			
	C (Pitch)	Da (Brightness	O (Pitch)	W
275	2.79	2.48	.58	1.44
550	3.65	3.13	1.06	2.75
1100	7.47	3.87	3.17	3.85

[48] W. Köhler, Akustische Untersuchungen, III, *Zeit. f. Psych.*, 72, 1915, 2.

[49] The data for this work with W, both numerical and introspective, have been lost, with the exception of what is here given.

Three other observers gave judgments which seemed to be concerned with the musical identity of the tones; C and O in the pitch-series, and Da under instructions for brightness. The limens so obtained are entered in Table XVII. The introspective views show the basis for classifying the judgments as those of tonality:

"Difference in pitch appears to be in relation to the key base created by the first tone" (C). "I do not get that higher or lower quality that generally characterizes pitch . . . When the tones are near together I get the 'out of tune' business" (C). "I still hear the tones as dependent on some key. Sometimes the first tone of a pair is tonic to the key in the light of which I hear the second tone; sometimes I hear the first in relation to a key" (C) "In looking for differences of pitch, there is undoubtedly a difference in brightness, I find; one tone is sharper and more lively than the other" (C). "Judgments made on basis of musical steps . . . a definite tonality often apparent, though unnamed" (O). "The semblance of musical interval was sometimes noted" (O). "At the lower pitches there seemed to be two tones, individuals in a musical sense, perhaps" (O). "Tonality established near beginning of series and seemed to persist throughout. That is, initial tone was of tonality; others were slightly 'off'" (O). "I try to abstract from pitch" (Da). "Tried to inhibit kinaesthesis so as to keep from making my judgments in terms of pitch" (Da). "Judgments very unsatisfactory. Have no basis" (Da). "Very successful in attending from pitch" (Da).

The musical character of the judgments rendered by C and O is quite clear. Indeed, C mentions differences in brightness, which he noted in addition to what he called 'pitch.' He had at that time no reason to suspect any identity of pitch and brightness. But, in view of the experiments cited earlier in this paper, one must necessarily identify C's 'brightness' with the pitch-brightness attribute, so that his 'pitch' can only be a musical characteristic, tonality. O, besides testifying to the musical nature of his judgments, specifically mentioned the influence of tonality, more fully justifying our use of these results under this heading. Da, on the other hand, gave only negative evidence. He interpreted the instructions for brightness as meaning that he should avoid judgments of pitch, and therefore cast round for something else in the tone which he might judge, *i. e.*, the difference which he found next after pitch. We cannot be sure that he was actually judging tonality; but the limens are so similar, both in magnitude and course, to those of the other observers that they are tentatively included here. It seems probable that, inasmuch as the observers were uncertain of their ground in making these judgments, the limens are not singly determined, but are cut across by pitch and are, therefore, only roughly figures for musical quality (tonality).

TABLE XVIII.

COMPARISON OF PITCH AND TONALITY LIMENS

Standard	Observers					
	Da		O		W	
	Pitch	Tonality (Brightness)	Pitch (Brightness)	Tonality (Pitch)	Pitch (Pitch and Brightness)	Tonality
275....	.85	2.48	.79	.58	.39	1.44
550....	.75	3.13	.69	1.06	.37	2.75
1100....	1.34	3.87	2.06	3.17	.70	3.85

Table XVIII gives a comparison of the pitch and tonality limens for three observers. In the case of O, the figures obtained under the instructions for brightness are taken as the pitch-limens, for his judgments under these instructions seemed to be made upon approximately the same basis as were the judgments of the other observers for pitch-brightness. Since W found that pitch and brightness were the same attribute, we have averaged the limens obtained from him under the two instructions to give a single set of values for pitch-brightness (which is called simply 'pitch' in the table). The table shows that, except in one instance, the limen is lower for pitch than for tonality. Moreover, its course is different for the two characteristics. The pitch-limen decreases slightly in the lower of the two octaves used, while the tonality-limen increases steadily throughout the whole range of our stimuli. So far as these data go, pitch and tonality are independently variable.

Our results are too scanty for any decision as to the final status of tonality. On the one hand, introspective descriptions of the bases upon which the judgments were made are lacking, so that it cannot be stated whether the musical quality here called tonality is an attributive or a perceptual phenomenon. On the other hand, the problem of 'octave-qualities' is yet untouched. There is nothing in the experimental evidence to indicate that an observer, when reporting that two sounds are or are not the same tone, judged a characteristic which repeated itself in every octave. In the absence of negative evidence, we may tentatively include tonality in our list of attributes, leaving a more extended investigation along the lines here suggested to a later study.

A further point is to be noted. In reporting upon musical qualities, one observer stated whether the second of the two stimuli was the same tone as the first or was *the tone next beyond it* in the one direction or the other. This is typically an ordinal judgment, while pitch brightness was always described in qualitative and never in ordinal terms. Tonality, then, and not pitch-brightness would satisfy Watt's concept of 'pitches' as 'orders.'[50].

VIII. Criticism of Previous Experimental Work

Köhler[51] found that his observers reported a vowel-like characteristic of tones of such nature that the pure vowels occurred exactly an octave apart. He failed, however, to show that this characteristic was attributive in nature, contenting himself with the fact that his observers could make vocal reports. Stumpf[52] repeated some of these experiments, and failed to find the same regularity of report. The same method of procedure was used in our own preliminary work in vocality, with the outstanding result that consistent vocal judgments were given only by those observers who were previously accustomed to some sort of work with vowels. It would seem possible, inasmuch as Köhler was working in a laboratory already interested in vowel-theories, that his observers came to the experiments with their perceptions of vowels ready-formed, while, on the other hand, some of those who took part in Stumpf's experiments as well as some who took part in our own had not reached this stage of development. Stumpf has further suggested that the 'pure vowels' which Köhler found were in reality a particular tonality (a "c-quality") which the observers had come to report as the turning-points of the vowel series. The work by the method of paired comparisions reported above bears out Stumpf's criticisms. The procedure was so arranged as to baffle any possible disposition to select a particular tonality as the turning point, with the result that the pure vowels did not fall an octave apart. If Köhler's data are the outcome of purely vocal judgments, there would be no reason for discrepancy between our results and his. We must therefore conclude that the octave relation was due to some factor other than vocality.

Four principal methods have been used to differentiate the tonal attributes: (1) phenomenological observation; (2) the study of pathological cases, including unmusical observers; (3) observation at the ends of the musical scale; and (4) studies of the attributes of noises and of very weak and very short tones. The first of these methods (the commonest and most primitive) can never give us information on the attributive status of a characteristic. The observations on pathological cases, as Stumpf has noted, yield results that are by no means unequivocal. In the particular case of experiments upon unmusical persons, there is no justification for saying that, because the differential limen of such subjects is high, it must be a limen for this or that attribute, unless we know that the limen for that particular

[50] H. J. Watt, *The Psychology of Sound*, 1917, 20 ff.

[51] W. Köhler, Akustische Untersuchungen, *Zeit. f. Psych.*, 54, 1909, 283 ff.; 58, 1910, 59 ff.; 64, 1913, 92 ff.

[52] C. Stumpf, Ueber neuere Untersuchungen zur Tonlehre, *Ber. u. d. 6 Kong. f. exper. Psych.*, 1914, 305 ff.

attribute in the normal observer is equally high The study of tones in the extreme ranges is fruitful in so far as it can show independent variability of attributes within the ranges, but it has the serious limitation that it tells nothing about their relation in the middle and more frequently used portions of the scale. A consideration of the characteristics of noise, as well as of those of very weak and very short tones, gives, on the other hand, evidence of inseparability; for if, whenever an attribute is missing from a sound, that sound must be classed as a noise rather than a tone, it is evident that the attribute in question is inseparable from tonal sensations. The difficulty, however, lies in the definition of noise. In one system of psychology 'tone' may mean a particular form of experience, while in another system it refers to any sensation resulting from regular periodic sound waves.

The methods of differentiation enumerated above either are phenomenological or, if experimental, are concerned with abnormalities, that is, with abnormal observers or unusual forms of stimuli. No experimental data obtained from normal observers in the middle range of the musical scale are offered to support the attributive distinctions. It was the aim of the experiments here reported to supply this deficiency, by applying the test of inseparability and independent variability under normal conditions. Such a test would seem preferable to any attempt to found a psychological system merely upon abnormalities and phenomenological observations.

It is worthy of note that the position taken by Stumpf in his paper before the Sixth Congress[53] in 1914 is in agreement with our experimental results. It will be remembered that he postulated three attributes: pitch or brightness, volume, and quality (tonality), and rejected vocality as not being attributive. Titchener[54] in 1915 takes a similar view of the problem.

Ogden,[55] partly on the basis of his experience as observer in these experiments, published in 1918 a revision of Watt's theory. He posits three attributes, pitch, volume, and brightness, defining the latter (in terms of Watt's theory) as the degree of emergence of the pitch salient from its surrounding volume. An earlier consideration of his introspective reports showed us that his pitch was in effect tonality, and his brightness pitch-brightness. We can agree, then, with his distinction of attributes, though not with the interpretation that he puts upon them. In particular, the concept of musical interval as being the result of volume-ratios is mathematically inaccurate.. If the differential limen for volume is proportional to the stimulus and if, further, the Weber-Fechner Law is accepted, then it must follow that the volume-*difference*, not the volume-*ratio*, is constant for the same interval at any point in the scale. This implies, of course, that the volumes of tones in octaves cannot be halved in size as one proceeds upwards in the scale.

Conclusions

1. As determined by the method of paired comparisons, the pure vowels do not occur at the same point for all observers, nor do they lie an octave apart.

[53] Stumpf, *op. cit.*

[54] E. B. Titchener, *A Beginner's Psychology*, 1915, 52 f.

[55] R. M. Ogden, The Attributes of Sound, *Psych. Rev.*, 25, 1918, 227 ff.

2. Judgments of vocality are made upon a perceptual and not upon an attributive basis. Some observers have these perceptions ready-formed from previous experience, while in others it is necessary to build up the perceptions before undertaking experimental work upon the vowel-qualities.

3. Judgments of pitch are made upon an attributive basis.

4. Judgments of brightness are similarly made upon an attributive basis, but are to be identified with judgments of pitch.

5. Previous experiments establishing the attributive status of volume, and showing that it follows Weber's Law, have been verified with pure tones.

6. It is possible for observers to report upon tonality in the sense of musical quality. These judgments may tentatively be accepted as indicating an attribute.

7. The three attributes, pitch-brightness, volume, and tonality, are independently variable in that: (1) their limens, within the range employed, occur in the ascending order, pitch-brightness, tonality, volume; and (2) the limen for pitch-brightness decreases toward the center of the scale and increases beyond it; the limen for tonality increases steadily, without following Weber's law; the limen for volume follows Weber's Law.

NOTE.—We should have explained above how it is that the observer *O* appears in the introspective reports of pp. 140 ff., but not in Table V, p. 138. *O* came into the experiment after the vocal series had begun. Since his experience in auditory observation made his reports of vocality especially valuable, we gave him the regular preliminary training, and then took with him as many series by paired comparisons and constant stimulus differences as were possible before the change was made to the attribute of pitch. The numerical results were not sufficient to serve as basis for the calculation of a limen.

SOME FORMS OF NATURAL TRAINING TO WHICH CERTAIN BIRDS ARE SUBJECTED

By P. F. Swindle, University of Missouri

On pages 274 and 275 of an earlier article on instinct and habit,[1] I discussed some natural training methods to which certain climbing birds are frequently subjected and which give rise in the case of the cockatoo to particular forms of behavior which I there referred to as 'cockatoo-2- and cockatoo-3-rhythms.' The training methods served, as the table on page 275 of that article shows, not only to rhythmize responses of long duration, but also to permanently isolate from them unitary groups and rhythmized series of movements of particular numerical values. I shall now turn to other birds, especially the carnivorous ones, and attempt to explain certain interesting regularities in their behavior. The investigations which I shall soon present consisted primarily in studying carefully the ordinary behavior of the animals under normal conditions.

The raven (Corvultur albicollis, Lath.): This is a large bird from East Africa which I observed carefully for more than two years in the Zoological Garden at Berlin, Germany. This bird cleaned its beak, as birds often do, by striking first one side and then the other against various objects of its environment. It did this at almost all times of the day, but unusually often after it had eaten a meal. Unlike many birds, however, this one frequently cleaned one side of the beak more thoroughly than the other. If the biological purpose in wiping the beak is to clean it, the animal should be expected to clean both sides equally well, unless something interfered with the perfect fulfillment of this noble purpose. Because no artificial training was applied to this bilaterally symmetrical organism to cause it to perform an asymetrical act, one may hope to discover the influencing factor or factors only if one carefully observes the more fundamental life activities of the bird which are conditioned by certain stimuli and then modified by certain other environmental forces.

In the case of this bird, the original response of striking the beak has been broken up into a number of fragments or uni-

[1] Ueber einfache Bewegungsinstinkte und deren kuenstliche Beeinflussung, *ueit. f. Sinnesphysiol.*, 1915.

tary groups of movements which contain from a single one to twenty-one perceptible elements. The expression '3-g' in the table which follows, means that the bird struck first one side of the beak, then the other, and then the first side again before it became active in some new way. The order of the strokes might be right, left, right, or left, right, left. Those groups, the initial elements of which were strokes of the right side of the beak, are indicated in the table as 'Right-sided groups,' while the others are given in the next lower row of figures as 'Left-sided groups.' The row of figures after 'Totals' shows, without regard to the nature of the initial strokes, the totals of the various quantitatively different groups. Fifteen months were required to obtain the data of the following table.

Group-names	1-g	2-g	3-g	4-g	5-g	6-g	7-g	8-g	9-g	10-g	11-g	12-g	13-g
Right-sided groups	4	7	50	20	52	22	55	21	28	11	8	40	1
Left-sided groups	12	27	380	66	396	290	471	55	183	173	45	120	36
Totals	16	34	430	86	448	312	526	76	211	184	53	160	37

	13-g	14-g	15-g	16-g	17-g	18-g	19-g	20-g	21-g
	1	21	14	4	2	1	0	0	1
	36	59	62	15	9	12	3	0	0
	37	80	76	19	11	13	3	0	1

It is easily observed from the table that the largest numbers of groups were those which the bird began by striking the left side of the beak. If this is a valid criterion for right- and left-sidedness, the figures show that the bird is decidedly left-sided. However, I could discern no clue to explain this fact. I was much interested to learn in this connection that other ravens of the same and other varieties which I observed were about as frequently right- as left-sided and that some were either-sided.

The bird's head gives immediately every indication of its prey. The most noticeable feature is its large, somewhat hooked beak which would seem to be well designed for tearing flesh. The truth is that this variety of the raven eats a large variety of things, such as fruit, grain, and flesh, but it is primarily a carnivorous scavenger. The birds of this variety frequently capture and devour small animals, but ordinarily they do not participate in this if they have access to a carcass.

The ravens, as well as all other flesh-eating birds, should be looked upon as typical grasping animals; for the activity of tearing flesh is essentially a grasping activity which is normally modified at intervals by the extra exertions of biting harder and by pulling and tugging.

I observed that when this particular specimen tears flesh from a carcass, it first takes hold of a piece of flesh (the first

perceptible element of the activity), and then pulls back (the second element); if the piece does not come loose, the bird gives an unusually strong tug which results as a rule in a pronounced movement of the head to either the right or the left (the third element); and if this strong tug does not free the piece from the carcass, the bird frequently lets go to secure eventually a better hold. The flesh seldom comes loose when the bird merely pulls back; the most effective exertion is the tug to the right or to the left. The left-sidedness of the bird is here manifested by its tugging harder and also more frequently to the left, while grasping the piece of flesh.

This behavior might enable one to account for the preponderance in the table of the 3- over the 2-groups.

The following form of behavior manifested in tearing flesh might likewise enable one to account for the unusual frequency of occurrence of the 5-groups manifested in the decidedly different activity of cleaning the beak. Whenever this particular raven retains a good hold on the flesh after the first tug to the left, it tugs next to the right (the fourth element), but by no means as strongly as to the left; and this time, if the grasping activity is not completely interrupted, as when the piece is freed from the carcass or when it slips from the beak, the bird next tugs strongly to the right (the fifth element). This extra strong tug frequently frees the piece from either the carcass or the beak, thus causing the grasping activity to be interrupted completely. Essentially the same statements can be made concerning the 7-, 9-, 11-, 13-, and other odd groups.

We should now try to understand why the 6-, 10-, 12-, and 14-groups appear, relative to the even ones, so much more frequently. So far, I have considered only those cases in which the bird manages to grasp the flesh firmly until one or another of the unusually strong tugs is exerted, and have not considered what the succeeding group might be if the bird should lose its hold at any time and again grasp and tug at the piece without becoming active in a new way in the meantime. If the piece should slip from the beak at the third, fifth, or seventh movement, the bird might again grasp the flesh immediately and in each case repeat the previous group. One should observe in these cases the 6-, 9-, 10-, 12-, 14-, 15-, and other large groups which are multiples of 3, 5, and 7. Observations show that although it is generally the case, this bird does not the second time invariably lose its hold or secure a piece of flesh at numerically the same movement at which it previously secured one, or at which it lost the same piece the first time. However, the same group of movements, when

numerically considered, is often observed to recur when the bird is attempting to secure a piece which it is not able to free by any amount of pulling and tugging. The necessary conditions are here provided for repetitions of groups of the same number of elements. The investigation described in the two following paragraphs shows conclusively that when an object at which the bird tugs is kept entirely constant, the animal does perform a series of quantitatively identical groups in attempting to free the piece.

After a few trials, I succeeded in regulating a strip of rawhide so that when the raven seized the free end of it, there occurred first the backward pull, and then the strong tug to the left before the piece was released. Frequently, the bird did apparently the same thing over and over without pausing. The groups generally contained three elements, and only a very few times two or four. I later adjusted also a tendon from a horse's leg to call forth in the raven apparent repetitions of the 5- and 7-groups. These groups were numerically the same, since all of them contained the same number of elementary movements, but the serious question arises here as to whether or not the same nervous structures which condition the muscular responses of one of the groups also function to condition the movements of any other one of the groups which are quantitatively the same. A large number of investigations which I have conducted on other animal subjects, as well as on human beings, yielded data which justify the statements that the nervous correlates of the different quantitatively identical groups are not the same, and also, that the nervous structures which function and condition one elementary muscular movement of a group are not involved in the direct conditioning of any other element of the same group; I have been compelled to conclude that the nervous structures which function at one time and successfully condition one muscular movement of a group conditions at the same time action in other nervous structures which, in functioning, condition in turn a similar muscular movement which is another element of the muscular group. The simple fact that an extra activity can be associated with a particular elementary movement of a group so that it is regularly conditioned by this element and not by any other one of the same group, is conclusive evidence that the nervous structures differ in the cases of the different elements of the group. It is this circumstance which makes possible a rhythmical series, i. e., a series of movements in which accentuated movements occur at regular intervals. These accentuated movements are ordinary elements which are accompanied by extra responses that are

responsible for the greater expenditure of muscular energy manifested at these respective points.

I attempted to adjust the tendon and also the raw-hide to call forth in the raven the 4-, 6-, and 8-groups, but my attempts were astonishingly unsuccessful. Apparently, this was due to the circumstance that the fourth, sixth, and eighth tugs which the raven gave were much weaker than the third, fifth, seventh, and ninth ones; and apparently, this in turn was due to the fact that the bird was left-sided, or at least that it was not either-sided.

An either-sided raven (Cervus corax, L.): It seems appropriate to give here the groups of another carnivorous bird of a closely related variety, a specimen of the European Raven. This individual differs from the first animal in that it is smaller, that it is either-sided, and that its beak is almost straight instead of hooked. Because the beak is not as well constructed for holding onto flesh that is grasped, this bird, while tearing flesh, seldom makes groups of movements that contain more than two elementary movements. These are the seizing movement and then the strong pull. If the piece does not come loose from the carcass when the bird pulls, it usually escapes from the mouth, because the straight beak is not well adapted for holding the flesh securely. The movements of biting and pulling often occur alternately for a considerable time before the bird becomes active in some new way.— The data presented in the following table were obtained in the course of six weeks.

Group names.....	1-g	2-g	3-g	4-g	5-g	6-g	7-g	8-g	9-g	10-g	11-g	12-g	13-g
The groups.....	3	70	34	105	18	38	15	21	7	7	3	5	0

This bird manifested an unusually strong tendency to clean both sides of the beak equally well, as is shown by the high frequency fluctuations of the groups containing even numbers of elementary movements. Unless some activity of ordinary life, such as that of tearing flesh, has modified the activity of wiping the beak, these fluctuations should not be so high. I shall suppose that this is the case, and to support the supposition, I shall present some typical groups which can be gained by observing almost any of the insectivorous and herbiviorous birds. For this purpose, I shall use the data gained from a finch.

The finch (Cyanospiza lechlancheri, Lafr.): This bird is a native of Western Mexico. It, too, is either-sided. The groups in the following table were obtained in the course of six weeks.

Group names.....	1-g	2-g	3-g	4-g	5-g	6-g	7-g	8-g	9-g	10-g	11-g	12-g	13-g
The groups.....	12	32	59	54	41	33	17	10	10	3	0	1	1

In the results gained from this either-sided bird, no marked fluctuations with respect to the occurrence of either the odd or even groups appear; and there really seems to be no activity manifested in the ordinary life of the bird to cause such fluctuations as the records from either of the previously examined birds show. The finch can scarcely be called a grasping bird, because its grasping responses are generally of very short duration.

The goose eagle (Gyps fulvus, Gm.): To return to the typical grasping birds, another interesting case of apparent transferrence of ability — that is, the capacity of an animal to manifest with an apparently untrained bodily member or members the effects of the training of apparently only a single member — is manifested by the larger sized eagles. Many of the adults seldom walk or run symmetrically—they gallop, so to speak. For example, the goose eagle, a native of Southern Europe and South Africa, in walking or running, often throws the weight of the body more on one foot; and the same individual may at another time give the other foot the greater responsibility. This alternate accentuation of the elements of the walking or running activity is the act of performing the 2-rhythm; and the activity of tearing flesh is obviously responsible for the development of this rhythm.

The bird is very strong, and, accordingly, whenever it seizes a piece of flesh (the first element) and then pulls (the second element), the piece comes loose from the carcass, slips to the extreme end of the hooked beak, or escapes from it entirely. In any case, the grasping activity is interrupted at the particular point at which the grasping response of long duration is otherwise modified to some extent by the introduction by the bird of the second element, i. e., the strong pull. If the piece is freed from the carcass when the bird executes the strong pulling movement, it is swallowed — which means that the eagle, in swallowing, becomes active in a radically different way, and that a pause for the isolated fragment of the grasping activity is thereby produced — and if the piece slips to the extreme end of the beak or escapes from the mouth entirely, the eagle takes a new hold immediately, thus producing a second time the same number of movements. The alternate seizing and pulling, and accordingly the periodic accentuations and interruptions of the grasping activity, often occur many times before the flesh is secured.

In the cases of some of the eagles, the fourth or hind claw of one foot is so long that people often advance the theory that for this reason certain individuals do not run or walk symmetrically; but carefully made observations yield data

which show that this is an erroneous supposition. The presence of an extra long claw on a single foot of an either-sided eagle, i. e., of an eagle which gallops by throwing the bodily weight more on the one and then after a time more on the other foot, shows that the abnormally long claw is at least in some cases not responsible for the characteristic galloping behavior. Moreover, members of the smaller or weaker varieties of the eagle do not gallop. It is also interesting in this connection that certain carnivorous birds with relatively straight beaks, such specimens as our crow and buzzard, often gallop similarly as do the larger eagles. These are birds of appropriate sizes and also with appropriately shaped beaks that they necessarily make groups of movements in eating flesh which are generally the same quantitatively as those made by the stronger eagles with the hooked beaks.

General Discussion

Generally, a person who is acquainted with the more important life activities of an animal and also with the mechanism, as viewed externally, for manifesting the activities of interest, can predict with considerable certainty the quantitative values of the groups which any given bodily member will manifest. But one who attempts to make such predictions would do well to consider that if any unforseen exception or 'mistake' occurs in the performance of any of the groups of the modified responses, it is seldom an underestimation, but generally an over-estimation most frequently by only a single element. This extra response is the one which was responsible for the isolation of the group from the response of longer duration, or, it is one which accidentally accompanied the inhibiting response. In either case, it accordingly became associated with the final element of the isolated group so that it is later conditioned by the final element and appears as a conclusion to the group in question. The extra response may or may not be similar qualitatively to the ordinary elements of the isolated group, as will be explained in the following paragraph.

It was always the case that whenever the ravens or the finch finished a group or rhythmized series, they became active in some new way. There is certainly nothing extraordinary about this simple fact; but it should be of considerable interest to us that groups of certain numerical values were frequently followed by very particular acts. One of the most interesting phenomena of this sort was manifested by the first raven. In approximately 40% of the cases, the bird bit and pulled the feathers of its breast for several seconds immediately after

the sixth element of the 6-group was executed. The bird did this also in conclusion to some of the other groups, but far less frequently than to the 6-group. For example, such an activity was the conclusion of the 5-group in approximately 2%, and of the 7-group in about 4% of the cases. The activity compound of wiping the beak six times and then biting and pulling the feathers of the breast, was not established under controlled conditions; but the circumstantial evidence in the case indicates that at some time or another the longer series of striking the beak was interrupted at or immediately after the sixth element by an itching of the breast which caused the new response. Perhaps the stimulus for the new response was a louse.

Sometimes, however, more troublesome movements of the head or of the body as a whole follow regularly, let us say the 4-group. Some of these are so simple quantitatively and so similar qualitatively to the ordinary elements of the group that even though the extra movement does not serve to cleanse the beak, an observer is nevertheless tempted to rank this movement with the elements of the 4-group and accordingly speak of a 5-group. This would be, however, a pure 4- and only a pseudo-5-group.

ANALYSIS OF NESTING ACTIVITIES

By P. F. Swindle, University of Missouri

A. Introduction

In earlier articles,[1] I used the term *Instinct* as meaning a series of innately associated simple reactions of an organism. A single movement is of course instinctive, because the necessary structures for its execution are present as an inherited part of the organism; and two movements form an instinct-group, if, before the animal's reactions are modified by training, the first element serves as a stimulus that regularly conditions the second element of the group. Such a series of movements should be called a unitary 2-group; one which contains five simple elements of movement would be a unitary 5-group, etc. The groups should be called unitary because their elementary movements are in each case very similar qualitatively, i. e., in the case of a given group the elements occur in approximately the same tempo, direction, and amplitude of movement.

A simple habit is such a simple group of movements which training has caused to occur more frequently than originally. The frequency of occurrence of a group can be increased only if one or more other responses of the organism are associated with and accordingly condition, as regularly as they occur, the initial element of the group in question. Through this process, an extra number of stimuli of the animal's environment may condition more or less directly a given group that we may at the time be interested in, and, as this number of extra stimuli increases, the frequency of occurrence of the group also increases.

It has occasionally been my experience that people who observed my animals and heard me use the word instinct, or who read my articles wherein I applied the expression to very simple responses consisting of one or a very short series of elements, seemed to think that the simple little groups in question should be designated by a different term. My earlier

[1] Ueber einfache Bewegungsinstinkte und deren künstilche Beeinflussung, *Zeit. f. Sinnespsysiologie*, 1915; The Term Reaction Time Redefined, *Amer. Jour. of Psychol.*, 1917; Relevant and Irrelevant Speech Instincts and Habits, *Psychol. Rev.*, 1917.

attempt, however, in experimenting with the simple group movements, was to study the elements which I presumed constituted the traditional instincts. For my own satisfaction I then synthetically constructed imaginary and really complex instincts out of the simpler ones. This was made as a stepping stone toward an intensive study of some of the traditional instincts. In this paper, I shall attempt to show that nest construction is the result of an interplay of instinct-groups, and after that I shall attempt to indicate the rôle played by the unitary groups in producing the actions which an animal manifests in defending its nest.

In the following discussion of the nesting activities, it will be of little significance whether or not the instinct-groups are in any case habitualized, in which case they would be habits as well as instincts; for the only difference which this could make would be a quantitative one. For example, if a unitary 11-group of a cockatoo should happen to be one of those which could aid in constructing a nest, and if it were so poorly habitualized that, e. g., the last three elements would sometimes occur independently of the preceding eight, the bird would do approximately the same thing in this case as when the group occurred as a whole; in the one case the act would be only of shorter duration and accordingly only quantitatively simpler. This would necessarily be the outcome of the partial occurrence of any group because the different elements of the group are very similar qualitatively.

B. Nest Building

Relation between bodily activity and complexity of nest: Observations justify the statements that, generally speaking, the nest-building animals are the most active ones of the animal kingdom, and that those which build the most elaborate nests are the most active ones of the nest-builders. When we review the next-building vertebrates we find that the birds are the most elaborate nest-builders; and it is to be borne in mind in this connection that the birds, as a class of animals, are far more active than any other class of the vertebrate kingdom. However, if the qualification for nest-building were no more than *activity*, we should expect the albatros to build an extremely elaborate nest; but it builds no nest. The albatros is very active, being almost continually on the wing, but it is after all active in a very few general ways instead of a great many. Mere activity is accordingly not sufficient; the animal must be active in very many ways.

The next most active class of animals is the rodents, and, it is interesting that the rodents build nests which are com-

parable in elaborateness to many of those constructed by the birds.

An examination of a relatively inactive class, such as the reptiles, gives us further facts which support our present thesis. Even though the greater number of these are egg-laying animals, they build very crude nests — if one may venture to call them nests at all. The alligator, for example, leaves the water for a short distance, entrenches itself — usually in the sandy earth — deposits the eggs, and then roughly and recklessly conceals them with earth and trash. For another example, the turtle digs a hole in the sand with its hind claws, deposits the eggs, and then covers them with earth and trash. These are mere holes in the ground; and I shall not reckon them as nests in the true sense of the word, since no foreign objects are collected and fitted together in any systematic fashion.

Relative utility of the various movements: It may be the case that many of the movements manifested during the process of building a nest have, when ontogenetically or phylogenetically considered, no biological purpose; and it may also be the case that many of those manifested at the time are even harmful in nest construction. When we are concerned with a very large number of activities of a given animal, the law of probability should enable us to predict that if a few of them are of extreme biological significance in nest-building, a corresponding few should be of very little value, if of any at all, or indeed harmful. If the outcome were just this, however, the condition for nest-construction would not be met. We should suppose that a nest can be constructed only under the condition that there is an interplay of great numbers of different groups, of which the useful ones by far outnumber the harmless and harmful ones. Careful observations show that an excess of very useful ones are manifested by the more elaborate nest-builders and fewer by the animals which built the cruder nests. A preponderance of useful groups of movements, where such exists, may be attributed to the biological factor of natural selection.

Theoretical conception of nest building: The lizard builds no nest, but for the present study it is without doubt an excellent subject for observation, because many of the easily observed groups of movements which it manifests are in no essential way different from many of the responses which a bird makes while building a nest. It seems as if the only possible reason why the lizard constructs no nest is that there does not exist a preponderance of responses which blindly

serve to construct a nest more rapidly than to destroy it. For this study, I shall take some of the activities of an Australian lizard, Bearded Agama (Amphibolurus barbatus Cuv.) and arbitrarily ascribe purposes to them which will enable one to easily imagine that the animal is engaged in the construction of a nest. After considering such a theoretical nest while holding the really purposeless movements of the lizard in mind, I shall examine the nesting activities of a bird, and determine the fundamental differences — if any are to be found — which may be discovered while comparing the two cases.

In the partial synthesis of the random movements of the lizard, I shall consider three purposeless activities — they are purposeless at least in regard to nest-construction. To each of these I shall assign fictitious purposes, such as would necessarily exist if the individual responses which make up these activities really served to construct a nest. These activities or complexes of responses are: (1) nodding of the head in the tempo of 0.30 sec., (2) nodding of the head in the tempo of 2.31 sec., and (3) inflating of the so-called beard in the tempo of 0.97 sec. For the sake of convenience I shall name these three activities A, B, and C respectively, and, for reasons that will be brought out later, I shall substitute the expression *complex* for the word activity that I have heretofore used. In assigning fictitious purposes to them, I shall call complex A, walking; complex B, seizing of objects with the mouth; and complex C, head movements which serve to arrange the objects to form the nest. At present, I shall not concern myself with the choice of the building place any further than to merely suppose that the lizard is to build the nest in the neighborhood of the place where it usually suns. The following are groups of movements that constitute the complexes A, B, and C, and which the lizard performed in the course of two hours:

Complex A....	-11	-11	-13,	11-	11	-	11	-	11,	13-	13-	13-	13-	13,	11	-	11-
Complex B....	-	6-	6-		-	6-		6-		-	-	-	-		6-	-	6-
Complex C....	-	-	-		-	4		-4		-4	-4	-	-4		-4	-	-4
		a	b	c	d	e	f		g	h	i	j	k	l	m	n	o

11 - 11 - p

The number 11, for example, signifies the number of elementary movements that constitute one of the unitary groups of complex A; the first group of complex B consists of six elements; and the first group of complex C is composed of four elementary movements. In the table, a dash means a pause of longer duration than fifteen seconds; a comma signifies a pause of less than fifteen seconds; and the absence of either a dash or a comma between the groups of the same or different complexes means the absence of a pause. The small arabic letters underneath indicate in cases single groups and

in other cases series of groups and pauses which we can conveniently use in our comparison. Neglecting the first long pause, the groups of the three complexes should be read, in the order that they were performed, as follows: (a) 11-group, no pause, 6-group, long pause; (b) 11-group, no pause, 6-group, long pause; (c) 13-group, short pause; (d) 11-group, long pause; (e) 11-group, no pause, 6-group, long pause; (f) 4-group, no pause, 11-group, no pause, 6-group, long pause, and so on.

This table of actual observations shows clearly that a quantitative expression can be applied to each group of a complex. It should be noted at this point that these unitary groups maintain their identity for a long period of the life of the organism. One evidence for this which can be gained by studying the table is that a group does not recur immediately; a recuperation pause of about fifteen seconds intervenes between two executions of the group.

According to the previous naming of the group-complexes, the significances of the various groups of the table would be somewhat as follows: (a) the lizard takes 11 steps, grasps an object which is fast, bites it 6 times, and makes a long pause; (b) 11 steps, grasps a twig which it bites 6 times, long pause; (c) 13 steps with the twig still in its mouth, short pause; (d) 11 steps farther with the twig, long pause; (e) 11 steps with the twig, bites it 6 times, long pause; (f) it moves the twig about on the earth 4 times, takes 11 steps farther, bites the twig 6 times letting it fall from the mouth, long pause; (g) without the twig it moves the head about on the earth 4 times, takes 11 steps toward the building place, short pause; (h) 13 steps toward the building place, and since this is only five steps away, it goes eight steps beyond it, long pause; (i) it moves the head against the bare ground 4 times, walks 13 steps farther, long pause; (j) it moves the head about on the naked earth 4 times, takes 13 steps, long pause; (k) 13 steps, lands on a large number of leaves without seizing any, long pause; (l) it moves the head about on the earth where no nest is, 13 steps, short pause; (m) 11 steps, immediately bites a leaf 6 times, long pause; (n) moves the leaf about on the earth 4 times, takes 11 steps toward the building place, long pause; (o) 11 steps toward the building place, arrives there and chews the leaf 6 times, long pause; (p) it makes 4 movements in arranging the first leaf for the nest, and immediately walks away to fetch another object.

Utility of a group dependent upon its proper temporal position: Even though definite purposes were assigned to the

various groups of the lizard, some of them did not always serve the intended purposes merely because they occurred at inappropriate times. The same thing can be said of many of the potentially purposeful groups of any given nest-building animal. During the nest-building season, one can frequently make the observation that a particular group is at one time purposeful, at another time merely purposeless, and at another time even harmful in so far as it serves to damage the nest which is in the process of construction. The movements which a bird manifests in the nest-building season are none other than such groups of movements which are characteristic of a great number of animals, for instance, in a degree with the lizard. The observed movements of the lizard were apparently purposeless, and even so are many movements of the birds purposeless in the same sense; and indeed a few are generally harmful, while a sufficient number of them are, however, purposeful enough by nature and so frequently occur at such appropriate times that a neat nest may be eventually constructed.

The fact deserves emphasis that birds often work very crudely while building the nest. It is really astonishing how often a bird allows objects of building material to fall, apparently without responding further to them. A bird frequently stands on or walks among objects which it could well use in constructing its nest but suddenly runs or flies away without grasping any of them. I have observed the Blue Jay to tear the leaves, branches, and feathers from another bird's nest before it seized an object of the foreign nest and flew to the one which it had started; and it often seemed to arrange the objects on the foreign nest as if it were preparing to deposit its own eggs there, which it did not do.

Application to nest-building of birds: In order to parallel the cases of many birds, the lizard was permitted to lose one twig and to make some unnecessary movements. By redefining the groups of the lizard, I attempted to ascribe a definite purpose to each of them, but I was not entirely successful in this attempt merely because the order and time of occurrence of some of the groups were occasionally such that the indicated purposes were thereby defeated. For instance, the movements which were so defined that they could serve to arrange objects for the nest, fulfilled this purpose only once in the course of two hours. The fact, however, that some of the movements to which particular purposes were assigned did not serve the intended purposes makes our theoretical case correspond more nearly to the real conditions of nest-

building; for, as will later be shown by a specific case, potentially purposeful movements of actual nest-builders often occur at such times that the normal or usual purposes are thereby defeated.

Even if the activities of the lizard were as purposeful as I made them out to be, the groups involved are so few and far between that nest-building on the part of the lizard would be an extremely slow process. But when one considers an animal with innumerable group-complexes which are the characteristic movements of the head, tongue, beak, tail, feet, legs, etc., one may begin to understand how a complicated nest can be constructed in a relatively short time. It can be observed in the nest-building season that groups of a few group-complexes, such as those of biting and arranging, occur unusually often.

As to why particular complexes of movements manifest themselves and then disappear suddenly, sometimes for a long period of time, is by no means easy to explain. One might say the growing eggs in the body of the female is a sufficient stimulus to cause her to become active in the proper ways to build a nest, but many males aid in constructing the nest. One might then say that the sexual responses are accompanied by a sufficient number of others to cause a bird to build a nest; but this does not help us any, because we should now explain the more or less sudden appearances and disappearances of the sex-responses. In connection with our present problem, the following facts should be given consideration: (1) Purposeful, purposeless, and harmful complexes often appear suddenly, after some time disappear, and later, after days, weeks, or even months, they reappear.[2] (2) Bird's nests which are constructed quite late in the summer are generally less elaborate than those built in the early spring-time. (3) Birds frequently build nests apparently for no purpose, that is, no eggs are deposited in them even though apparently nothing occurs to prevent their doing so; they may be in and on the nest as frequently after it is finished as before, which fact would seem to indicate that they do not fail to deposit the eggs in the nest because some man or beast disturbs them. For some unknown physiological reason group-complexes appear and disappear.

Nest building of the Cariama (Cariama cristata): I shall next examine the activities of a bird in the nest-building sea-

[2] On page 261 of my article, Ueber einfache Bewegungsinstinkte, etc., already referred to, I reported a case of the sudden appearance and the later disappearance of a purposeless complex of a lizard. Many other similar cases have come under my observation.

son in order to compare these with the responses of the lizard which were previously presented. For this purpose, I have chosen a specimen of the Cariama, a native of Brazil and Paraguay. The particular bird observed was the male of a pair in a large out-door cage in the Zoological Garden at Berlin, Germany. Early in April I noticed that as this bird walked about in its cage, it occasionally bit in the air as if it were grasping an object. At times, however, it bit the bars of its cage, a branch of a tree, and even the naked earth. Sometimes it sprang and ran about rapidly, and it very often flew to a one and a half meter post on the top of which a wide shallow basket was fastened. On April 18, 1915, the following movements were observed: The bird stood at first motionless in the basket, shortly it began biting in the air as if attempting to seize something, and occasionally it seized, lifted, and then dropped certain branches which lay in the basket. It threw a stick out of the basket onto the ground, gazed for a few seconds at the sky, flew down, ran a few steps, seized a twig, walked to the post, grabbed still another twig which it had previously thrown out of the nest, flew back into the basket with these, beat them quickly here and there without releasing them, let them fall in the basket, bit and arranged them there, and then remained for many minutes by the side of the female which was then also in the basket. Presently the male, half-springing and half-flying, reached the ground, ran quickly to and fro in the cage, gazed for a while at the female as he walked round and round the post, and finally sprang and seized a twig which projected from the basket. This twig was unfortunately so badly tangled with the others of the nest that a great many were drawn out with it. But in spite of the fact that the nest was occasionally mutilated by the builder, a neat nest was eventually constructed.

Simple and multiple nests, or *serial character of nesting activities:* In the case of many birds, only very simple nests consisting of a single kind of objects, are constructed. In many cases, however, the nests are multiple ones; at first an ordinary simple nest is constructed and then on top of this one another simple one is constructed of another kind of building material, and so on until in cases the completed nest consists of a half dozen or more such simple ones. Owing to the fact that the building material and texture vary in the different cases of the simple nests, the analysis of such a multiple nest is easily made. The first simple nest of a multiple one may consist of large sticks; a second, of smaller sized sticks; a third, of mud; a fourth, of grass or fine roots; a fifth, of hairs or

fine fibers of plants; and a sixth, of feathers. I have observed considerable variations in the order of the simple nests which were constructed consecutively by the same birds. I have produced certain pronounced variations in the multiple nest by destroying it as soon as the first, second, or third simple nest was constructed. After a few days, in cases after only a few hours, the birds again started building either at the same or at some new place. In many cases, the new multiple nest did not contain any of the elements of the parts which I tore away; that is, the first one or two or three simple nests were omitted in the second attempt at nest construction. In a few cases, the birds continued to collect objects, but upon reaching the tree from which the previous nest had been torn, dropped these one after another until enough material for the characteristic multiple nest of these birds had been brought and dropped. When this happened, the birds generally waited a few days and then began a new nest which, when completed, included all the simple nests as usual.

The results of this experiment seem to indicate that the first simple nest is due to the presence of certain group-complexes, many of which disappear more or less completely giving way in each case to other complexes which enable the birds to construct the other simple nests in the characteristic order. In the case of a given variety of bird, such group-complexes manifest themselves in a particular temporal order, each one or several simultaneously occurring ones lasting for a few days and then disappearing for days, weeks, or even months. Just as a simple group of one or more elements cannot continue indefinitely, without the intervention of pauses for the structures that function, the complexes of groups cannot continue indefinitely when the pauses between the groups are made quite short; one group after another drops out until the greater part of the complex has disappeared. After the simple nests of a multiple nest are constructed, there are no complexes which might prohibit the female from sitting on the eggs or the male from singing. It must be borne in mind in this connection that sitting on the eggs is an activity as is that of building a simple nest; and it is more obviously the case that the singing of the male involves the manifestation of group-complexes analogous to those which serve to construct the nest.

After the eggs are hatched, or after the female has set on them long enough for them to be hatched, new group-complexes manifest themselves. In some cases — as with the English sparrow or the goose — these complexes serve to continue the construction of the nest after it contains the eggs.

The sparrow, for example, carries feathers and other objects of building material and completely covers the nest of eggs or young. If the young birds were to eat these objects of building material, we should say that the parent birds were merely feeding the young. It so happens that in the case of the sparrow, the young birds do devour a large number but by no means all of the objects brought to them. Those objects which the nestlings eat are insects, worms, and the like which would serve to form a covering for the nest if they were not devoured. The goose covers its nest with nearby objects every time it leaves it, and brushes these aside with its head, feet, breast, and wings when it returns. Many birds do not cover the eggs, and many do not even feed the young, which facts are to be contrasted with the facts that many birds build no nests or very simple ones while other birds build elaborate multiple nests.

Selection of the building site: My intention was to include in the present division of this article only a very general discussion of nest-building. I did not give a lengthy account of the process of selecting the building place; for such would have to be modified for almost every species of the nest-builders. Many birds construct their nests in chimneys, under eves of houses, in cavities in the banks of streams, cliffs, and trees which they frequent. By supposing that the lizard was to construct its nest in the neighborhood of the place where it usually sunned, I attempted to make its case typical of those of the birds just referred to. To a certain degree the lizard, in selecting a place for its theoretical nest, was also allowed to typify a large number of other birds which build their nests in trees; for it can be said that these birds, also, build their nests in the neighborhood of the places which they frequent. It should be remarked, however, that they do not frequent the particular mats of branches and the forks of trees which generally hold the nests, as much as they do other parts of the trees.[3] But this can be accounted for by the fact that, as a rule, only these portions of the trees are adapted to support the nests. It is evident from observing birds that it is not the sight of an ideal building place which stimulated them to begin the nest; they commence by carrying the objects of possible building material into the trees, often permitting them to fall to the ground merely because these do not happen to lodge on the particular limbs on which the animals happen to stand. Many birds can build their nests at only certain portions of the trees which are adapted to hold the collected

[3] Those birds which build swinging nests afford a possible exception.

objects; and generally, these places are located by the birds only after a number of trials. That this fact is not well known seems to be due to the circumstance that it is very difficult to observe a bird with sufficient scientific accuracy in freedom. It should also be mentioned in this connection that birds occasionally start two or three nests simultaneously and later destroy some of them to obtain the material for a single nest. It often happens that these places which are well adapted to hold objects of building material, contains a last-year's nest, in which case the old one may serve as a support for the new nest.

It is to be observed that when a bird flies to a given tree, it usually lights on a *certain* branch and then hops, walks, or flies to other parts of the tree. If the nest is already started at any time when the bird lights on this 'favorite' branch, it is stimulated by the sight of the collected objects to make the same group of movements which it performed in passing the first time or first few times from this branch to the building place. When it lights on some new branch of the tree, it may perform the same habitual group of movements in passing through the tree; and if this does not lead it to the nest, the bird may stand still for a short time — as if in a state of meditation — and shortly release the object which it has brought, or perform a new group of movements which may take it closer to the nest. The started nest becomes accordingly a stimulus to call forth in the bird certain responses which cause it to carry still other objects from its usual places of lighting in the tree to the place to which the objects were earlier carried.

C Defense of the Nest

Character of defense reaction: When a bird is approached by a person or some other intruder before it has begun to construct its nest, it usually flies away without emitting any special sounds or showing any signs of an instigated emotional state unless it be that of fear. But, after the foundation of the nest is laid, the bird shows signs of distress when approached. It may hop or flutter about in the neighborhood of the nest, all the while crying or at times even attacking the intruder. This activity of distress, like all other activities, does not continue indefinitely. The bird eventually shows less signs of emotion, in that it cries less frequently and finally leaves the nest entirely. If the intruder remains near the nest until this happens, it is usually the case that the bird 'quits the nest,' that is, it responds to it no further.

If, after a long pause, the bird returns after the intruder

has left, it may respond to the nest somewhat as usual, but also simultaneously to the intruder as formerly. In the absence of the intruder, however, the emotional state of excitement is not as great as in his presence; but it is frequently observed that when the bird does return to its nest the first, second, or third time after the intruder made his appearance, it cries somewhat as it did the first time in the presence of the actual intruder. Upon the return to the building place, the nest alone serves as a comparatively insignificant stimulus to call forth a large number of the responses which were previously called forth by the combination of nest and intruder, and which became associated in a temporally superimposed order while the bird was being stimulated by this heterogeneous stimulus pattern — in short, a fraction of the objective stimulus which previously affected the bird now calls forth fairly distinctly the previously established pattern-response. Such is generally the case if the bird returns at all before it has completely 'quit the nest.' In some cases, the bird may, while responding to the intruder in his absence, even mutilate the nest which it has built. I have occasionally observed birds which, upon returning to the nest after the intruder was no longer visible to them, broke their own eggs or slayed the young.

With a few birds, the unusual state of powerlessness or merely behaving exceedingly awkward is manifested when an intruder approaches them. As a rule, such actions can be observed only when the nest is entirely constructed and eggs or young are in it. Also, such actions are to be observed in the complete absence of the nest but always when the parent bird is with the young. As to whether the bird becomes unusually powerful or unusually powerless, depends upon the nature of the responses which are at the time called forth simultaneously; if the majority of them are allied, the animal may be temporarily powerful, and if the majority of them are non-allied responses, it may be temporarily powerless.

It is always to be noticed that after the parent bird leaves the nest or young for a considerable distance, it ceases to fight or even to walk or fly awkwardly. After it has gone a considerable distance from the nest or young, it responds to the intruder by flying away, as at ordinary times; it does not respond so strongly to the combination of nest or young and intruder when the nest or young are absent. At a later meeting of the same intruder, birds are frequently observed to respond to him somewhat as they did to the pattern-stimulus of nest and intruder or young and intruder. This corresponds to a bird's responding to the nest as if to the combination of nest and intruder. In each case, a part of the previous stimulus

calls forth many responses of the pattern-response which was previously established by the combination of nest and intruder.

Reaction against intruder dependent upon degree of completion of nest: This subject has been touched upon in the foregoing paragraphs, but a more systematic discussion of this point seems advisable. While carrying the first few objects of building material and arranging these in the tree, certain habits become established. These habits are groups of one or more complexes which for some reason or another appear at this particular time and which, being called forth frequently by a large number of external stimuli, are on the road to temporary disappearance. The intruder together with the part of the nest which is constructed, serve as a pattern-stimulus to call forth a very large number, if not all the groups of the complexes simultaneously. The nest serves to hold the bird in the vicinity of the building place in spite of the presence of the intruder; and the intruder serves to cause the bird to respond in a very large number of ways in a relatively short time. As was previously emphasized in the present article, any act of relatively short duration can recur only after the lapse of an adequate recuperation pause for the organic structures that function to produce the elements of the response. It was also pointed out that if the adequate recuperation pause is in any case made as short as possible, the response will ultimately occur much less frequently than before the animal was subjected to this treatment. Accordingly, when a large number of the defense-responses occur simultaneously — and the remaining ones in a rapid serial order — and the intruder remains present until the groups begin to occur very infrequently, the bird naturally ceases for the time being, if not permanently, to respond in the excited way to the combination of nest and intruder.

If two or three simple nests of the multiple one have been constructed, it is much more difficult to cause the animal to desert the nest permanently. It is also to be noticed that the state of excitement which an intruder can then cause in the bird is correspondingly greater than that which is manifested after only the first few objects have been collected and arranged. This is really what one should expect, because, when the nest has neared completion before the intruder appears, the bird is molested only after several complexes — those necessary in the construction of the various simple nests of the multiple one — have come into play. We should not forget that these complexes did not disappear completely but should remember that after a time the groups of the complexes

only occurred less frequently than when they were serving the purpose of nest-construction. The groups of the different complexes can still be manifested, and of course simultaneously, provided the stimulus is heterogeneous enough for this purpose. It is important that we consider in this connection only those complexes which have shortly before appeared and then apparently disappeared; for, while any given complex was the predominating one, its various groups became highly habitual and did not cease entirely to be habits. Accordingly, in view of the supposition that a habit-group is only an instinct-group which is called forth by a number of accidental stimuli as well as by the original most adequate stimulus for the response, we may suppose that a given group of an apparently disappeared complex is more likely to be called forth by the combination of nest and intruder than if the complex had never been a predominating one. If a group has never been habituated, or, if it has been a long time since this happened, such a pattern of stimuli, as that of the nest and intruder might possibly not contain the proper or adequate stimulus for calling forth the initial element of this group directly. As the building progresses, the number of groups of the various group-complexes which appear and disappear in a serial order, and which the combination of nest and intruder may later call forth simultaneously, increases to such an extent that the emotional state of excitement produced by the heterogeneous pattern is much greater than one which the same intruder could help to instigate during an earlier stage of nest-construction.

THE PERISTALTIC-LIKE NATURE OF ORGANIC RESPONSES

By P. F. Swindle, University of Missouri

A Introductiion

The expression *peristalsis* is traditionally applied to the wave-like movements of the intestines. When a living intestine, e. g., the small intestine, is stimulated at a certain point, one observes first a wave of relaxation and following it a wave of constriction. Normally, this double wave travels along the intestine in a particular direction. It passes from the duodenal toward the caecal end. This peristaltic movement may be looked upon as a response of long duration, the greater part of which is a positive after-effect of the stimulus or force which conditioned the initial portion of the response. For the execution of each portion of the peristaltic response, organic structures function which are not directly involved in the execution of any other part of the response; new sets of structures come into play one after another until the entire intestine has reacted. One set of structures functions, conditions action in the neighboring structures which have not reacted immediately before, and then undergoes a recuperation pause before it functions normally again. If one stimulates the intestine a short distance behind a peristaltic wave, one can thereby produce a response at that point but this stimulation ordinarily does not give rise to a peristaltic wave; instead, ordinarily occur only the well-known churning movements which usually take place about a bolus that continually stimulates the intestinal wall.

Peristalsis is also characteristic of the vascular system. In the frog or turtle, the contraction appears to start in the sinus venosus and to travel from there in a definite direction to more distant portions of the system. Peristalsis in the insect heart has a much greater resemblance to that of the intestine.

Peristaltic-like movements of the lungs of certain animals, e. g., of the frog, are easily observed. If one stimulates the end of a half-inflated frog lung, a characteristic wave of peristalsis will pass from the point of stimulation to the other end of the lung.

The points of special interest derived from a study of the above mentioned structures are:

1. Different organic structures are involved in the execution of different portions of the responses of long duration.
2. One set of structures conditions action in neighboring structures which have undergone a recuperation pause.
3. The peristaltic response can be started at any point along the intestine. If a point midway between the duodenum and ileocaecal valve is stimulated, the response originates and progresses from the mid-point to the caecal end only.
4. Muscular structures can function for a considerable time without pause, as is illustrated by the churning movements which can be induced behind the peristaltic wave. In view of the circumstances that non-recuperated muscular structures can execute such pronounced movements, would seem to indicate that the nervous system of the intestine, perhaps the plexus of Auerbach is necessary for the transmission of the peristaltic wave and that after a given portion of this plexus reacts it can function normally again only provided that an adequate recuperation pause intervenes.

If it were the case that nerve fibers, which we could call a, b, c, d, e, f, etc., left the intestine at intervals of one centimeter and terminated in such a bodily appendage as the arm, we should expect a movement of the hand to occur as often as the peristaltic wave passed over these points. If this were the case, we could name the movements of the arm a, b, c, d, e, f, etc. Moreover we should expect that if a particular, extra activity were associated with c, a similar one with f, etc., the hand would beat a 3-rhythm. The extra responses would cause the c- and f- responses to be different from the others, e. g., b and d.

My attempt in this paper will be to show that the theoretical nervous and muscular mechanism of the above paragraph is in a high degree similar to nerve-muscle complexes in general. I shall deal with the unavoidable conclusion that a nervous structure which functions and conditions a muscular movement does not do exactly the same thing again before the occurrence of an adequate recuperation pause, not for the muscular but for the nervous structure; instead we must conclude that the nervous response, besides conditioning the muscular movement, conditions also a response in adjoining nervous structures which in turn excite a similar movement of the muscles, and so on.

The assumption of such nerve-muscle mechanisms seems to be a necessary prerequisite for explaining the existence

and behavior of the unitary group which I have discussed at considerable length in some of my previous articles.[1] The assumption of peristalsis in the nervous system should enable us to realize it is significant after all that animals ordinarily possess millions of nerve fibres which our systems of thinking have heretofore not required as necessary for ordinary forms of behavior. In other words, our assumption furnishes a possibility of using for theory an entire complex nervous system.

B The Unitary Group a Fragment of a Longer Discontinuous Response

The unitary group may be defined as *a number of qualitatively very similar movements which are innately associated and which accordingly induce or condition one another in the particular temporal order in which they occur.* The elements of the group are qualitatively very similar because they are approximately the same in complexity, in direction, and in amplitude of movement—in short, they are qualitatively very similar if any one of them seems to be a facsimile of any other element of the group.[2] For an illustration, I selected the group of Fig. 1 because, in spite of the circumstance that the amplitudes of movement vary in this exceptional case, the direction and apparent complexity of the five elements are nevertheless so nearly the same that they seem to belong together as a unified whole. The groups of Figs. 2, 3, and 4, also are characteristic unitary groups.

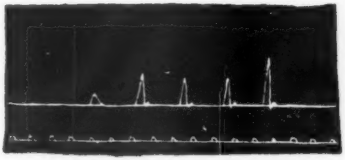


Fig. 1

Fig. 1 is a group of innately associated elements which a cockatoo produced by striking a tambour with its beak. The animal was not trained to produce these simple movements in

[1] Ueber einfache Bewegungsinstinkte und deren kuenstliche Beeinflussing, *Zeit. f. Sinnesphysiol.*, 1915; Mechanische Rhythmem bei dem Menschen, *Zeit. f. Sinnesphysiol.*, 1916; The Term Reaction Time Redefined, *Amer Jour. Psychol.*, 1916; Visual, Cutaneous, and Kinaesthetic Ghosts, *Amer. Jour. Psychol.*, 1917.

[2] In the case of the group which contains only a single element, this can of course not be said.

this particular order. For this reason, the series may be called an 'instinct-group;' and further, because it contains five elements, it may be conveniently called a '5-instinct-group' or simply a '5-instinct.' While the amplitude of movement of a group is generally quite constant, it is nevertheless more variable than either the tempo or the direction of movement and is accordingly the least significant criterion of the unitariness of the group; for it seems that no animal is capable of performing a series of movements of absolutely equal amplitudes. The amplitude is in any case more easily influenced than either the tempo or the direction of movement. The problem of the cause of these variations in amplitude of the movements within a given unitary group is identical with that of the cause of the final accent which will be thoroughly discussed in this paper.

The unitary group of Fig. 1 maintained its identity in different environments because, in general, its first element conditioned the second; the second, the third; the third, the fourth; and the fourth, the fifth element. In other words, it can be said that *it maintained its identiy in the different environments because it occurred more frequently as a whole than as fragments of the whole.* The group, as a whole, occurred more frequently than one or all of its parts without the whole because the adequate stimuli for the initial element occurred more frequently than did all those for the other four elements of the group.

Relationship between instinct and habit: A given group can occur more frequently as a whole, thus maintaining its identity when the animal is subjected to different environments, if especially the initial element is associated with the various other responses which condition it every time *they* occur. When this state of affair exists, however, the series is not only an instinct-group but also a 'habit-group;' *a habit-group is only an instinct group which occurs as a whole more frequently than originally.* The process of converting an instinct-group into a habit-group is a process of training. It may sound paradoxical that an instinct-group must be made habitual before it can be treated as a unitary group to which a particular quantitative expression is applicable; but, as was previously suggested, the training merely causes the innately associated series, that already exists, to occur more frequently as a whole than as possible parts. Fig. 2 will serve to make this point clear. The cocktoo produced these groups also by striking the tambour with its beak.

These two unitary 5-groups were associated in such an

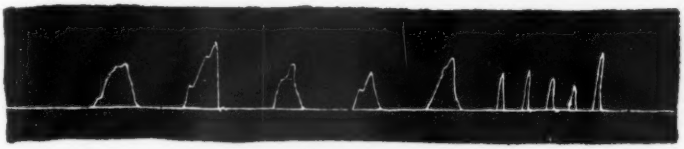


FIG. 2

order that the final element of the first conditioned regularly the initial element of the second group. Both of these were instinct-groups, and *both were at the same time habit-groups*, otherwise neither of them would have maintained its identity in the different environmental conditions as both of these really did. The second group was, however, more habitual than the first, because its initial element occurred now whenever the final element of the first group occurred to condition it; *the second group occurred more frequently than the first, because the first group was the cause of the second and the second group was never the cause of the first one.*

Fig. 3 is essentially the same as Fig. 2, even though it may seem at first sight, as though we were concerned with a 5- and a 6-group instead of two 5-groups as in Fig . 2. The second group of this figure is not the first one repeated with

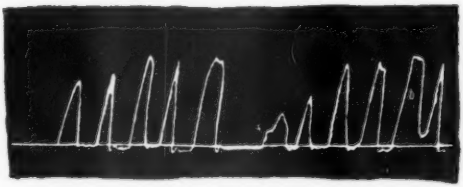


FIG. 3

slight modifications; groups which are of as short duration as these, do not recur until a pause of several seconds elapses. Observations show that a group of very long duration may recur without the intervention of a long pause; as a rule, the longer the group the sooner it can recur after the final element is executed. This can only mean that the structure correlates of the initial elements of the long group become recuperated while the succeeding elements are being executed.

The final accent: It may be noticed that the final element of the first group of Fig. 3 is more elaborate than any other

element of the same group — in short, it expresses a greater expenditure of muscular energy than does any other element of the group. The final element of the second group is still more elaborate; and in this case, it is easily analyzed into at least two distinct movements. It consists of the usual stroke of the beak against the tambour followed by an unusual stroke with one side of the beak, the latter being responsible in the kymograph record for the distinct appendage of the fifth element. The fifth element plus this appendage constitutes the *final accent* of the 5-group. Without this extra appendage, the fifth element is, like that of the first 5-group, accentuated—the word accentuation implying that the element in question is a compound consisting of two or more different activities—however brief these may be. The final element of the second 5-group is, because of the extra stroke, more complex than the final element of the first 5-group, except when we have special reasons for considering the second 5-group as a constituent response of the final accent of the first group.

Elements of the unitary group associated in one direction only: It is a fact of considerable significance that the elements are associated in a definite direction. When the first element occurs it conditions the second, the second conditions the third, and so forth until the fifth element of a 5-group is executed; but, when the fifth element is the first to occur, it does not condition any other element of the group. Also, for example, when the second element occurs first, it conditions the third and not the first element of the group. The association of the elements in a single direction is a fact which can be demonstrated very clearly and easily by the proper use of 'discrimination activities.' For instance, just as a cockatoo was executing a certain element of a group, I stimulated it in such a way that it uttered a brief cry at the same time; and, while it was executing the next element of the same group, I presented the appropriate stimulus to cause it to shake its head slightly. These brief extra activities of crying and shaking the head became associated with the two respective elements of the group and were regularly conditioned by them in the same order in which they were caused to occur in the training. The element which conditioned the shake of the head, occasionally occurred without being preceded by that one which the training caused to condition the crying; but, when the other element was produced first, it was regularly followed by the second one which could be identified by the extra or discrimination activity associated with it. Another demonstration of the association of the elements of a group in one

direction only, is the fact that the second of the two associated groups of either Fig. 2 or 3 sometimes occurred alone while the first group was in either case regularly succeeded by the second one. In each of the cases mentioned, the second 5-group is a discrimination activity, the first element of which is associated with and conditioned by the final element of the first 5-group.

Different organic structures are involved in the execution of the different elements: The fact that an activity, such as that of the crying of the cockatoo, is conditioned by a given element and not by any other one of the group, is a logical demonstration that the various elements are behavior correlates of organic structures which are in no two cases exactly identical; in other words, it is evidently not the case that the entire set[2] of organic structures, which is directly responsible for the initial element of the group, functions repeatedly to produce each of the succeeding elements. With that set which is involved in the execution of the element with which the activity of crying is associated, one or more organic structures are present which are not found in a set involved in the execution of any other element of the group; otherwise, the act of crying would be conditioned also by other elements of that group. The structure correlates of the activity of crying are associated with some of those particular structures which do not function in the execution of the other elements. If the elements of a 5-group are merely the initial one repeated four times, the crying would be necessarily conditioned by all the perceptible elements of the group instead of by the very particular one with which the crying was associated in the training.

The unitary group is a fragment of a longer series of innately associated elements — The complete instinct: My observations and experiments on trained and untrained organisms have led me to conclude that each unitary group is only a fragment of a quantitatively larger series of innately associated elements. It is a fragment which is either completely or incompletely isolated from the innate series of a larger number of elements which present a progressive qualitative change from the initial to the final one — *such a series is a complete instinct, and any fragment of it is an instinct-group.* Whenever the initial element occurs, the series runs its natural

[2] The term 'set,' as here used, merely signifies that many nervous and muscular structures are involved in the execution of a single element of the group.

or innately determined course, unless the organism is in the meantime acted upon by an external or internal[3] stimulus or force which conditions a response that completely interrupts the series in question. Since the qualitative change is a gradual one, any relatively short discontinuous fragment of the series is a group of qualitatively very similar elements.

The response which occurs at the point of interruption of the instinct becomes associated with the final elements of the totally or partially isolated group. If, while a member of the body is performing a series of innately associated movements, sufficient physical strength is exerted—for example, by another person's grasping and holding the beating member—to interrupt this series just after the fifth element is executed, a unitary fragment or a unitary 5-group is isolated from the series. If the interrupting force occurs frequently at this point and each time calls forth a response which would otherwise not accompany any element of the series, a unitary group of muscular responses becomes accordingly established as an independent response. What is more, the very force *that causes the complete isolation of the 5-group, gives rise to the extra muscular response which becomes associated with and is later conditioned by the fifth element.* The extra response may, however, be of such short duration and of such a nature qualitatively that it does not inhibit the remaining part of the series, under which circumstance a unitary 5-group is established without being permanently dissociated or isolated from the series. When such extra acts are caused to occur at regular intervals, the series merely becomes rhythmized.

The forces which call forth the extra activities, need not act directly on the beating member; they may act upon it in an indirect way by affecting first the nervous structures which, in the strict sense of the word, *condition* these extra muscular movements. This is what more generally takes place. Whatever the nature of the interrupting forces may be, which are applied to the organism while the series is in progress, the essential thing is that at each of the presentations one or a number of activities are called forth and complicate an element of the series.

The foregoing observations and experiments of this article justify the following statement which may be used as a principle in interpreting modified responses. *Any one of the extra muscular responses which is responsible for the accentuation of a movement or which is merely conditioned regularly by*

[3] An internal stimulus might be a hormone. See discussion on p. 208.

the final element of a group is either a consequence of the force which directly or indirectly interrupted the series of innately associated elements, or it is the very response which caused that force to be applied to the already acting organism. This principle is applicable to all cases of unitary groups, whether or not the groups in question have been completely isolated from the longer series. It means the same thing to say it is applicable to all cases of final accents or concluding responses, independently of whether these are of short or long duration.

If the extra activities are brief ones which occur in the training immediately after the fifth elements and before the succeeding elements of the original response are executed, these extra movements will form in the kymograph record, appendages comparable in each case to that one which immediately follows the fifth element of the second group of Fig. 3. An examination of ordinary human activities does not enable us to understand why the bilaterally symmetrical human being should beat so frequently the 3-rhythm, unless we find or suppose that such extra movements regularly occur to give to each real 2-group of some of the 2-rhythms, which are developed in ordinary life, the appearance of containing three elements. If, however, the extra movements should be caused in the training to occur simultaneously with every second element of the series, the groups should later not be overestimated by a single element; there would be developed real 2- instead of pseudo-3-groups.

Fig. 4 is an illustration of a large part of a complete instinct in which many incompletely isolated groups are manifest.

The original record from which this figure was taken is 62.5 cm. in length. The chronoscopic markings below the movements occured in the tempo of 0.2 sec. The entire series of movements of which five parts are here given, endured 6 min. 17.8 sec. This is approximately the time which elapsed between the execution of the first movement in the lower left hand corner and the last one in the upper right hand corner. In examining the movements, one should begin with the lowest of the four rows and read from left to right. When the test was made, the subject merely knew that the finger was moving against a tambour.

The qualitative change manifested in Fig. 4 is a progressive change of tempo. Another series may show only a change of amplitude, a change of direction, or a change of tempo, direction, and amplitude of movement. As was previously stated, it seems to be impossible for an organism to produce a succession of beats of equal amplitudes; there is always at least a slight variation from movement to movement. Some

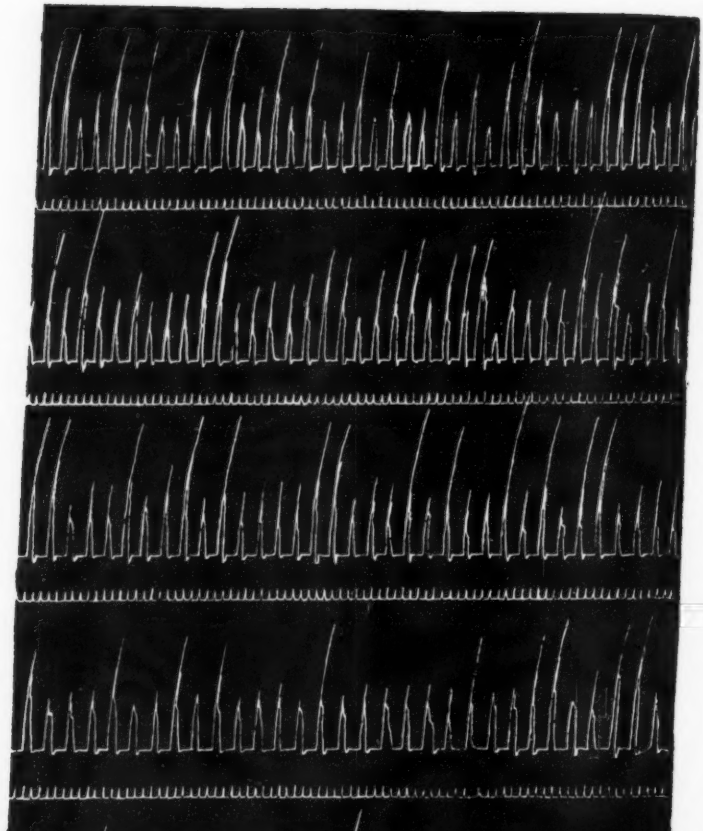


FIG. 4

of the movements of this series indicate a greater expenditure of muscular energy than do others; and it is of interest here that each of these was made strong by a very special training. Any one of the accentuated elements represents an extra amount of muscular energy simply because the training served to compound it and at the same time leave its neighboring elements relatively simple. Each accent consists of a number of activities, viz., the original element of the series plus one or more extra activities, any one of which is analogous to the cry or the head movement of the cockatoo, that served to modify a certain element of the group, as was discussed on page 192.

The art of reading kymograph records is difficult to learn; but this particular record is so simple that no one should encounter any special difficulty in attempting to follow the 5-rhythm through the series. The most essential caution which I can give the reader, who wishes to trace a particular rhythm in the record, is that he must learn to pay attention to final accents and not to look for initial or intermediate ones; what may at any time seem to be the initial or intermediate accents are the final accents of groups of other rhythms and should be accordingly overlooked while any particular rhythm is being traced. One must also bear in mind that if any errors occur in the production of the rhythm, these are seldom under-estimations but are almost invariably over-estimations of the group by a single element. It should be noted, however, that these are only apparent over-estimations; for what really happens is that a final accent of a group is at such times easily analyzed into two distinct movements, as is true of the final accent of the second 5-group of Fig. 3. When one takes note of the precautions here stated and reads from left below to right above, beginning anew at each of the five rows of movements, one easily finds that every fifth element of the series is accentuated; one who observes this, observes that which may be called a psychological 5-rhythm in the series.

After the 5-rythm has been traced, one might trace also the 7-rhythm which started later in the series. It is present first in the second row of movements from the bottom; and it should be no more difficult to find it than it was to re-discover the 5-rhythm in the same row. After this second task is accomplished, the 11-rhythm, which is present first in the third row of movements, may be traced. If these three attempts at tracing rhythms do not involve all the accentuated movements, it is advisable to try to detect still another rhythm. But, if no other one, whose groups are numerically different

from any of those already discovered, can be traced, it is well to search for a second 5-, 7-, or 11-rhythm, the accents of which are not in all cases coincident with those of any one or all of the rhythms already traced. Two non-coincident 7-rhythms are present in the fourth row, and two non-coincident 5-rhythms are to be found in the upper row of movements. It is easily observed that with the increase in tempo the accentuated movements become more numerous, which fact is due to the circumstance that not all the rhythms started simultaneously but at different points of the series. This particular series of movements does not happen to contain a second 11-rhythm.

In my many pounds of kymograph records of the movements of trained animals and human beings, it is unmistakably the case that many of the learned rhythms are sometimes duplicated and even tripled in the same series. This was the case even though the training was at no time purposely designed to produce this result. This fact shows that a training which causes an organism to beat with a given bodily member one rhythm of a particular numerical value, trains it at the same time to beat with the same member other rhythms of the same numerical value.[5] Sometimes only one of these occurs alone, while at the same time two or more of them occur in a temporally superimposed order, in which case their accents may or may not be coincident. Of course if all of the rhythms of a certain numerical value should occur simultaneously and accordingly have their accents coincident, the analysis of the amalgamation of activities would be so difficult that in reading the kymograph record we should generally recognize only a single rhythm. By introducing discrimination activities, however, such as the crying or the extra head movement of the cockatoo, and associating these with appropriate elements of two 5-rhythms, for example, which can be caused to occur simultaneously both of these rhythms can be detected in the series where otherwise only one may be judged to be present. This experiment is possible only because either one of the two rhythms, which may occur together, occasionally occur separately. When they occur separately, the two qualitatively different discrimination movements can be associated with two numerically corresponding elements; and, after the associations are established, the person, who is well acquainted with the activities of the organism in question, can present the appropriate stimuli to cause both of the rhythms to start simultaneously. In this case, the two discrimination acts occur

[5] When the unitary groups of two or more rhythms are numerically the same, I shall speak of the rhythms as numerically identical ones.

together and thus furnish conclusive evidence of the presence of the two rhythms.

Apparent transference of ability: We have seen that *a training which causes an organism to beat with a given bodily member a rhythm of a particular numerical value, trains it at the same time to beat with the same member, simultaneously or at different times, other rhythms of the same numerical value.* It is also the case that *when the training is designed to teach the organism to beat a particular rhythm with a given bodily member, all other members are at the same time trained to do numerically the same thing.* A human being who is trained to beat with his foot the 5-rhythm, for example, is later capable of beating one of the same numerical value with his head or hand. A cockatoo which was trained to beat a 5-rhythm with its head, frequently beat a 5-rhythm with its foot, which it moved back and forth past its beak.

I have records of many such transferences. These are, however, only *apparent* and not *real* transferences; *for the training is generally not limited to a single member, but involves the entire body.* It is really not correct to say one particular member is beating alone merely because it is striking something or only moving in the air; for the entire body is active in numerically the same way at the same time. It may be at one time the hand, at another time the head, or at another the foot which is the most active; but it seems that whenever one of these members is making strong movements, the others are also active in numerically the same way, even though perhaps very much subordinated. Anything that occurs to alter one or more elements of the series which the hand is producing, also alters a temporally corresponding element of the simultaneously produced series of the foot or head. To generalize, *any stimulus which affects the organism while a series of one bodily member is in progress, and calls forth a response which becomes associated with an element of this series, generally calls forth a similar response which becomes associated with and thus causes corresponding elements of the other bodily members to be also accentuated.* And *what is true of the complex organism is true of any complex portion of it, such as the hand, foot, or head.*

The unitary group consists of a pattern of allied responses: One important reason for entering here into the discussion of certain facts of rhythm production, is to show that any element of a series is generally, if not in all cases, a *pattern* of simultaneously occurring responses. Accordingly, when one speaks of a series, or of any fragment thereof, one must not

forget that it is a pattern of many similar activities. With this point in mind, it will be profitable to discuss still some other facts of behavior revealed by well-trained organisms.

To refer again to Fig. 4, either one of the rhythms there present may occur without the others, as the presence of only the 5-rhythm in the initial part of the series indicates; but, when the 5-, 7-, and 11-rhythms occur simultaneously, as is the case later in the series, accents seem at first sight to occur in a lawless or haphazard fashion. After these rhythms occur simultaneously many times, however, they appear, as a rule, to occur together hereafter, i. e., in later tests. It is frequently the case that they occur together, but it is sometimes only an appearance. The fact that every fifth, seventh, and eleventh element is accentuated does not necessarily mean that the original three differently rhythmized series are present at the same time. For instance, that series which was rhythmized by having its every fifth element accentuated, may be entirely absent; but it may appear to be present merely because certain of the constituent parts of its accents have been transferred to numerically corresponding elements of other series present in the amalgamation of activities. *A single one or several of the activities of brief duration which were, in the training, associated with the fifth element for instance,* and caused it to indicate in the record a greater expenditure of muscular energy than its neighboring elements, which were not affected in the training, *became associated with and were accordingly later conditioned by the numerically corresponding element of at least one other series that previously occurred in the amalgamation of rhythms.* The revelation of this mutual transference of constituent parts of accents of one series to numerically corresponding elements of entirely different series, was the result of the application of intentionally chosen discrimination activities in experiments designed to determine the pattern-like nature of a given series, which was for various reasons supposed to be an amalgamation of a number of qualitatively very similar or allied responses.

Dominant and subordinate groups can often be detected in each pattern response: Because of the above discussed mutual transference — which means the 'adoption' by the elements of the different simultaneously occurring series of some of the constituent parts of accents—there is eventually after long and persistent training, no indication of dominance or subordinacy of any one rhythm of the amalgamation. At the first few noticeable cases of temporally superimposed rhythms, a single one of them is generally dominant in expression over

the others; they may be alternately dominant and subordinate, but they are as a rule not equally distinct at the same time. The only criterion of dominance and subordinacy of the rhythms, is the relative strengths of their accents; that rhythm with the most pronounced accents is the dominant one. *When the 5- is the dominant and the 7- the subordinate rhythm, this merely means that the number of 5-rhythms in the amalgamation exceeds that of the 7-rhythms. If only a single 5-rhythm should be present, every fifth element of the series of movements would condition fewer of the extra activities to increase its strength than would be the case if many 5-rhythms were occurring simultaneously.* This is equivalent to saying the simpler a movement is the fewer activities it conditions; or, the compound movements condition the greater number of extra responses. Let us say that when a single 5-rhythm is present, every fifth element conditions five brief responses, and that when ten 5-rhythms occur simultaneously, every fifth element conditions ten times as many or fifty brief responses as the contituent parts of any accent. It is very likely not the case that the simpler element conditions exactly one-tenth the number of brief responses induced by the specified compounded movement; but the only point which I wish here to emphasize is that the more complex element should condition the greater number of extra responses, and should accordingly indicate, for this if for no other reason, the greater expenditure of muscular energy.

As a conclusion of the preceding paragraph, the general statement may be made that *dominance vs. subordinacy of an activity means the occurrence of many vs. relatively few allied or qualitatively very similar responses.* The term allied responses refers to those which are so similar qualitatively to one another that they can easily occur simultaneously.[6] It is not necessary that the discontinuous responses in question be rhythmized; my only purpose in rhythmizing any series of movements was to train the organism in such a systematic fashion that I might be enabled to analyze a given amalgamation of activities into its various components.

The natural tempo of a group changes to match that of a simultaneously occurring and dominant group: A careful study of systematically trained animals reveals the fact that simultaneously with the occurrence of a larger number of responses, which are allied to the extent that they have the same characteristic tempo, a relatively small number of responses of one

[6] I mean to use the term 'allied responses' strictly in the traditional sense.

or more other characteristic tempos, can occur; but, in such a case, the tempos of the latter are modified to match that of the larger number of allied responses. *The tempo of movement of the smaller number of responses is determined by that of the larger number of allied responses of the amalgamation.* This means that *although the tempo of a dominant response is extremely constant and difficult to alter without interrupting the series entirely, as a subordinate response its tempo is easily altered.* The fact that in many cases two sets of non-allied responses occur simultaneously, can be clearly demonstrated by the use of properly chosen discrimination activities. For example, the two groups of Fig. 2 were caused to occur simultaneously [7] by presenting simultaneously appropriate stimuli for the initial elements of those groups. There occurred at such times a temporal correspondence of the elements as was shown by the fact that when the group of the slower tempo was dominant, a discrimination activity — a grunt-like noise uttered by the cockatoo — which was previously associated with only the final element of the other group, was executed. This can mean only that the relatively few responses which formed the subordinate group of the amalgamation, sufficed to condition the vocal response. Experiments with the internal accents of quantitatively the same and different groups yielded analogous results.[8]

We may speak of the thus altered tempo of the subordinate group as a *forced tempo.* And the fact of the forced tempo enables us to better understand why it is that an organism which is trained to beat with a given bodily member, for example a 5-rhythm in a given tempo, is later capable of beating numerically the same rhythm in all other tempos and with all other bodily members. Stimuli which are presented while a series is in progress and cause given elements of the dominant response to be modified, affect similarly the numerically corresponding elements of the one or more subordinate responses of the amalgamation. The subordinate responses of this amalgamation will later occur as dominant ones in their ordinary or characteristic tempos, and will show the numerical modifications that were brought about while they were subordinate in the amalgamation of activities which was affected by the training.

An established pattern tends to shatter: Our view of the

[7] This experiment was performed many months after Fig. 2 was made.

[8] Such an experiment with the same cockatoo, I reported in an earlier article, Ueber einfache Bewegungsinstinkte und deren künstliche Beeinflussung, *Zeit. f. Sinnesphysiol,* 1915, S. 287-288.

subordinacy and dominance of groups enables us to understand immediately the fact that while a dominant group of relatively short duration does not recur as such until a pause of several seconds has elapsed, it *apparently* recurs immediately as a subordinate group. The fact seems to be that the large number of allied responses which occurred together as a dominant group at the time of the training, do not become so permanently associated in the simultaneous order that they must always occur together. A few of these which were modified in the training, may later occur in the absence of the majority, and accordingly leave the impression on an observer that the dominant group may occur immediately as a subordinate one. This may happen in the case of long rhythmized series; in which case, we speak of alternately dominant and subordinate rhythms.[9]

B. Grasping Responses

As is indicated by the following investigations of the grasping activities of crayfishes, turtles, babies, and birds, these responses also are subject to essentially the same laws of behavior as those obviously discontinuous responses already discussed. Grasping responses too, are discontinuous.

The crayfish: The young crayfish grasps with its feet and clings to the chitinous hairs which fringe the swimmerets of its mother. The large claws or chelipeds are the principal grasping organs; and I shall consider these to the exclusion of the walking legs, the first two pairs of which are also grasping appendages. With a single cheliped the young crayfish can support, according to my rough estimations, from one to one hundred times its own weight; in fact, while supporting a heavy weight, the member sometimes snaps off at its base if left grasping the object for a short interval. For the reason that it is inconvenient to work with such a small and delicate animal as the young crayfish, I shall consider only the grasping activity of the adult. The adult, when agitated, grasps an object which stimulates the sensitive hairs of an open cheliped; but, if the object is a rigid one, it is very soon released. However, I have observed crayfishes to cling with a single cheliped to a soft watery weed or to a piece of flesh for an hour or so.

I did not find it agreeable to allow many of the larger-sized crayfishes to pinch my finger; but those large ones which had molted only shortly before, caused me no pain. I accordingly

[9] For such alternately dominant and subordinate rhythms, see my article, Mechanische Rhythmen bei dem Menschen, *Zeit. f. Sinnesphysiol.*, 1916.

worked more extensively with these. I permitted a large soft-shelled animal to grasp my index finger just a short distance behind the root of the nail; and I noticed that during this act of long duration, the animal pressed unusually hard with an extreme regularity in the tempo of 0.73 seconds. This was exactly my pulse rate. Apparently every time my finger became enlarged slightly with blood, the crayfish was thereby stimulated to pinch harder. The behavior of the animal at each of these accentuations within the grasping activity, showed clearly that it exerted its entire body at these intervals. Many other crayfishes, however, made such exertions at quite irregular intervals; they did not seem capable of pinching more strongly in the rapid tempo of 0.73 seconds. In such cases, I introduced a regularity of the accents by squeezing my wrist until I had counted each time ten pulse beats. Every time I released my wrist, the crayfish in pinching more strongly folded its abdomen or tail more tightly, moved its walking feet in various directions, and attempted — as it seemed — to reach my finger with the other cheliped. After I had thus stimulated a crayfish at every tenth pulse beat for a large number of times, I observed that when I purposely failed to release the wrist at the tenth beat, the crayfish behaved as though I had done so; it pinched harder and at the same time executed the other movements mentioned. By this means, I rhythmized an apparently continuous response; for when I allowed a thus trained animal to grasp a piece of frog, turtle, snake, snail meat, or even a piece of watery weed, it responded similarly as when my finger was the object grasped. When I used fresh frog, turtle, or snake meat, I found it much easier to observe the periodic responses of the crayfish because each of the strong exertions of the animal frequently called forth a decided contraction of the muscles that were being pinched.

Pauses between experiments necessary to conserve the subject: My experiences with the crayfishes, lobsters, and crabs have shown me that if one works unceasingly with one of these animals, it eventually becomes a worthless subject; one must make pauses at short intervals, giving the animal a long rest at each time—otherwise it will soon cease to grasp at all. In order to work extensively with a single animal, I allowed it to grasp for a period of not over thirty seconds before the long pause was made. My attempt to thus conserve a given subject for a large number of experiments, gave rise to the experiment of the following paragraph.

In order to bring about the pause, I did not pull the animal

loose from the object which it was grasping but dipped its tail in water, touched it on the ventral surface of the abdomen, or suddenly blew my breath onto its body. I caused an animal to release my finger at the thirtieth pulse beat. After many such ordeals, I found that the animal released my finger at this point in the absence of the interrupting stimulus. By this means, I accordingly isolated a fragment from the response of long duration. It is interesting that in certain cases of well-trained animals, this isolated fragment was rhythmized; it terminated just as the third unitary group was completed. To be sure, the elements of the unitary groups could not be perceived as a number of beats; but they were present because the grasping member was exerting itself during the while and an extra response, which was conditioned at one point of the series, was not conditioned at any other point of that series. Three different accents occurred at very definite points of the series. A demonstration that these were at least not in all cases the same extra responses repeated at regular intervals, is that the third one inhibited the grasping response completely while the others merely accentuated certain elements of it. *In the sense that there is a qualitative change from point to point of the grasping response, the activity can be said to be a discontinuous one.* As was demonstrated in these experiments, pronounced beats can be inserted along the response to make it resemble the obviously discontinuous ones which were considered earlier in the paper.

It is really a very easy matter to train a crayfish to release objects very soon after they are grasped. I found this to be so very easy that I had to use a number of precautions in order to conserve the subjects for such investigation as those above reported. Instead of saying that I taught an animal to release an object immediately after grasping it, I should say that *I broke the activities of long duration up into fagments of such short duration that the animal supported its weight for only a very short time.*

The turtle: Experiments with 'snapping-turtles' revealed essentially the same facts. I observed a large turtle while it dug with its hind feet in the sandy earth on the bank of a stream. After the cavity was finished and after it had deposited eighteen eggs in it, I appoached the animal. It lay as if dead until I pulled its tail, after which it hissed and snapped viciously at me whenever I made a slight movement. When I jumped behind it, the turtle snapped at me before taking time to turn the body. At such times the head hit the back of the shell with such force that it quickly rebounded against the

sand in front of the animal. I allowed the turtle to bite a piece of leather, which was my belt. It clung so tightly to the belt that I could sling the heavy animal round and round me without causing it to release its hold. I fastened the belt to the branch of a tree by hooking the buckle over a broken limb, and left the turtle hanging. Shortly after I hid behind a thicket of bushes, the turtle released the belt and fell. After I had done this about two dozen times, the turtle had apparently lost its ability to grasp the belt for any great length of time; for in my presence, it thereafter released the belt after a very short interval of grasping. But the ability was not completely lost; for when I set the animal swinging, pulled its tail, or 'jabbed' it on the soft parts at regular intervals, it clung to the belt for a long time. Moreover, after I had removed all its viscera, thus leaving the mere hull of shell and musculature, the turtle also clung to the belt for a considerable time.

At another time I found two turtles copulating. Because the male seemed to be the more vicious of the two, after they were separated. I worked primarily with it. By jumping at it, by striking at it with my hand, or by blowing in its face, I could induce it to snap at me. In some mud I found a straight tree branch which was decayed and soft on the outside but solid in the central region. I enticed the turtle to bite this. The animal supported its weight for seveal minutes at a time when I agitated it during the while. I cut a tin can so that it was cup-shaped, cut a round hole in the bottom, and through this I passed the end of the branch which I then allowed the turtle to bite. After the turtle had supported its weight for a period of twenty seconds, I let the can fall down the stick, thus covering the entire head of the turtle. On such occasions, the thus hooded animal frequently responded by moving its feet and tail rapidly in various directions, and in a few cases it released the stick. I prepared another can which was so light and shallow that it, when dropped down the stick, generally called forth the movements of the feet without causing the animal to lose its hold on the stick. I then dropped this can at intervals of five seconds, thus causing the distinct movements of the feet and tail at five-second intervals. After I had repeated this ordeal about six hours, the can became a superfluous stimulus to call forth the feet and tail movements; that is, the turtle responded at approximately five-second inervals as if to the falling can, but in the absence of this stimulus. The grasping activity which, in the case of the agitated turtle, was of very long duration, became rhythmized.

I next built a fire and heated the larger of the two cans. After the animal had supported its weight for about fifteen

seconds, I slipped the heated can over the opposite end of the stick and allowed it to fall down upon the turtle's head at the end of the period of twenty seconds. The turtle released the stick almost immediately. After I did this a large number of times, the turtle generally fell at the end of the twenty-second interval, independently of whether the can was dropped or not. The errors which occurred were generally slight over-estimations of the twenty-second period. By this training method, I isolated a relatively short fragment of definite duration from the response of long duration. What is more, the segment which was thus isolated, was rhythmized; for the turtle, because of its previous training with the smaller can, moved its feet and tail in approximately five-second intervals while the segment was in progress.

I worked with still other turtles in the same way and achieved similar results. I found, however, that it was not necessary to heat the can; for when I filled it with tobacco smoke, which lingered in the over-turned can, and allowed it to fall down the stick the animals released the objects as readily as when the can was heated.

In the case of the turtles, it was necessary to make long pauses occasionally; otherwise they, like the crayfishes, would cease to grasp or at least not support their weight a sufficiently long time for my purposes.

The human being: The grasping activity of the human being seems to be essentially the same as that of the crayfishes or turtle. The newly born child grasps and clings to an object which stimulates the palm of its hand. If the object is light, the child throws it back and forth without releasing it; which fact indicates the analytic nature of the response. But if it cannot move the object, as when the arm is stretched in supporting the body, the act of long duration seems to be a continuous one. Any stimulus which interferes so seriously with this response that the hand releases the object, causes the child to become active in some new way. This new or extra activity is the immediate cause of the interruption of the responses of long duration; it isolates a fragment from the response in question and is later conditioned by the latter part of the fragment which it isolated. In the course of time, the grasping response of the child, like that of the crayfish or turtle, becomes broken up into so many relatively short fragments that the child almost invariably releases the object after a very short time; and we can then say that the grasping activity has degenerated. The ability to grasp and support the weight of the body is, however, not lost; for even the

adult can do so when properly stimulated. I shall suppose that he can support his weight for a considerable time when appropriate stimuli are presented to call forth stimultaneously and successively an unusually large number of the isolated fragments. It seems plausible that the most adequate stimuli for this purpose are some of the hormones—secretions of the ductless glands of the body—which are thrown into the blood under conditions of excitement. It is perhaps for this reason that the crayfish, turtle, or human grasps for an unusually long time when agitated.[10]

The grasping activity is only one of great numbers of responses of long duration which become broken up into relatively short fragments during the period of early infancy. The disintegration seems to occur to some extent before birth, as is indicated by the fact that some newly born infants either do not grasp strongly or soon release the object grasped unless they are in a state of emotion at the time.[11] Human infants are, as a rule, subjected to various sorts of treatment by adults; and, as a rule, the environment of one child is otherwise rarely identical with that of another. This means that human infants are trained in many different ways. They receive such unusual treatment that we should expect the idiosyncrasies of the human to be much greater than in the case of almost any other animal species. Even the children of a single family are not subjected to the same training; the various responses of long duration are in one case broken up into fragments which are re-combined with the allied and non-allied groups to form different activity compounds from those of a brother or sister. If all the children of a family were born in a litter and subjected to the same environmental conditions during the early period of infancy, as are young bees, ants, wasps, or birds in a nest, we should expect the activity compounds manifested by the individuals later in life to be practically the same.

[10] This theory is worked out in somewhat greater detail in my article on Relevant and Irrelevant Speech Instincts and Habits, *Psychol. Rev.*, Oct. 1917. The subject of discussion there was the fact that stuttering and stammering activities are unusually prevalent when the individual is in a state of excitement.

[11] See John B. Watson and J. J. B. Morgan, Emotional Reactions and Psychological Experiments, *Amer. Jour. of Psychol.*, XXVIII, 1917, pp. 170-171. "In many cases we find that the child either cannot at first support its full weight with either right or left hand, or if so only for an instant. If, however, we first produce a state of rage in the infant by hampering its movements we rarely fail to get it to support its full weight for a considerable time.

C Conclusiion

Throughout the previous discussions, I have given reasons for supposing that all muscular responses of long duration are discontinuous in the sense that the nervous correlates of a limited portion of such a response are not exactly identical with those of any other portion of the same response. Assuming then the discontinuity of all the muscular responses or the persistaltic-like nature of responses in general, and speaking only of the behavior correlates of the structures that function,[12] the following law of induction can be stated: *The initial element of the innately associated series of elements of the responses of long duration conditions or induces its qualitatively most similar element; this in turn induces its most similar element which has not occurred immediately before, and so on until the qualitatively most dissimilar element to the initial one is induced.* This law is essentially the same as a law of color induction which I stated elsewhere[13] as follows: *In successive and simultaneous color induction, any color induces first itself and last of all its antagonistic color.*. The only difference between the two statements of the same law of behavior is that in the one case, emphasis is laid upon the qualitative natures of the muscular movements which succeed one another, while in the other field of behavior, the succession of psychical states only are brought into consideration.

If all the quantitive expressions were eliminated from the previous discussion of obviously discontinuous responses, still more completely than was done in the case of the grasping activities, and if the term 'pattern of visual responses' were consistently substituted in the *proper* places for the term 'pattern of muscular responses' wherever the latter is written or merely implied, that discussion would be thereby converted into a fairly complete theory of visual instincts and habits supported by the facts of vision. Special caution should be used, however, to make these substitutions at the *proper* places; for in a discussion of visual responses, certain muscular responses should be taken into consideration. These are the ones which carry the body through space or merely cause the fixating organism to change its point of fixation, thus causing the retinas to be stimulated in new ways while any given visual response is in progress. These extra muscular movements are responsible for the isolation of fragments

[12] I do not intend to present a physiological theory of induction at this point of the paper; and shall therefore not formulate a strictly physiological law of induction at this time.

[13] Positive After-Images of Long Duration, Amer. Jour. of Psychol., 1916, XXVII, p. 332.

from the visual responses of long duration and the consequent shortening of the original visual series; they become associated with, and are later conditioned by the fragments which they caused to be dissociated from the longer visual series. It is primarily because of these particular muscular movements, which have previously modified the visual responses, that perfect fixation is such an extremely difficult task.

We should expect that if kinæsthetic and visual responses are governed by the same laws of behavior, these laws should be likewise applicable to cutaneous, olfactory, gustatory, and auditory responses. My researches indicate very strongly that this is the case; and, in closing, I wish to express my conviction that the system of thinking brought out in this article is especially significant for a theory of auditory instincts and habits.

SOME RELATIONS BETWEEN THE WAR AND PSYCHOLOGY[1]

By G. Stanley Hall

There is a large sense in which psychological forces play the chief rôle in all wars. It is they that cause war, that carry it on, that make or wreck morale both in the army and at home, that determine victory or defeat in the field, and that are or ought to be the chief heirs of all war's results. Only when we understand and learn how to control them can the world be safe for peace. This is the goal to which we are slowly progressing, and though it is yet far off it was never so clear as now.

In the more definite field of psychology in the technical sense this country has set the world a new record in utilizing our science for military efficiency, and not only their colleagues but the nation and the world will realize more and more as the years pass, and the history of their various lines of activity is written up and evaluated, the homage due those who contributed to this great epoch-making work. Only when the history of American psychology is recorded in large terms will we realize the full significance of the work itself and the self-sacrifice with which it was undertaken by those who left academic halls to serve their country in utilizing psychology to help the war — Yerkes, Dodge, Scott, Thorndike, Watson, Stratton, Baird, Terman, and a long list of others who have so greatly extended the influence and increased the public service of our science. The time has not yet come to do full justice to this kind of work.

I am still less capable of treating my theme but can only tab off, without trying to amplify, a few impressions as follows:

(a) There is a strong tendency after such a cataclysm to revert to first principles, to ask ourselves what human nature really is and also why civilization, which is only man's attempt to domesticate himself, does not seem to fit his nature or needs; and therefore how to so form society as to make human institutions, like the clothes in Carlyle's "Teufelsdröckh," fit better. We know that very slight if constantly acting psychological

[1] Read at the Twenty-Seventh Annual Meeting of the American Psychological Association, Baltimore, December 27, 1918.

causes accumulate their effects and may cause great upheavals after generations, as e. g. some ascribe human migrations to slight salt shortage; and so in the social body, just as in the individual, even slight misfits between what is and what should be, between fact and wish, may produce serious results. Thus there is a tendency now to reevaluate, analyze, and reeducate the public soul which should be taken full advantage of.

Why is man such a fighter? Why all these reversions to primitive instincts against better insight, interest, humanity, religion? Only by knowing causes can we effect cures. Is not the persistence of war in the world a reproach to psychology as well as to sociological and economical insight?

(b) I think applied versus pure psychology has abundantly justified itself in this war. If there is such a thing as pure psychology, as we speak of pure mathematics, it is an iridescent dream of a goal that is yet very far off, or like the conception of pure soul apart from the body. Its practical domain is daily life. Psychology lives, moves, and has its being in explaining how and why human beings, especially man, sense the world and think, feel, and act in it, and our goal is to control these processes.

A generation ago it was a reproach to apply psychology even to education; now it is happily quite proper to apply it to business, love, religion, mental hygiene and the rest, and there is no concession of culture to *kultur* in so doing for in all these fields we have found vastly more facts than we can explain. Our science is in its dawn, and we have been mistaken in following analogies of physics and chemistry and seeking for psychic elements. We must not forget Aristotle's warning that it is amateurishness to affect a greater degree of exactness than the subject matter of science admits of. Biology, rather more than the sciences that deal with non-living matter, should be our basis, or more especially zoology, which explains the life of animals.

Our science is essentially pragmatic. Its criterion is service. Its supreme end is to teach how to get the most and very best out of life. It may help every science but it can never aspire to be a science apart and by itself, and its history is strewn with the wreckage of futile efforts to attain finalities and hyperexactitude. All such efforts help for a time but in the end retard. They make us feel that we have arrived when in fact we have only just started. Ours is the highest, largest, and most complex of all the sciences and will be the very last to approach completeness, which indeed it can never attain until man ceases to develop.

(1) The only purely pure psychology is that of ghosts—ghosts that have not even an astral body but the demonstration of whose existence has been complete as guaranteed pure from matter as if by some Lipmus test. They must not have corporeity enough to be blown about on a windy day like Plato's discarnate souls, they cannot be hermetically sealed up, and they may not even be limited to tridimensional space.

We cannot disembody souls in the laboratory, and so pure psychology seems to be next-world psychology. Moreover, if our souls, à la Plato, freely chose our bodies and our careers before birth it would be in some sense a violation of contract to investigate these purely spiritual bodies until after we die. If all the millions of souls freshly dead are organizing to cross the bourne from which no traveler returns, as Conan Doyle, Sir Oliver Lodge, and yet more tediously our own American Hyslop say they are doing, we ought to be receptive and show a decent loyalty to these battalions now organized to conquer the laws of matter themselves, and be ready to at least meet them halfway in their efforts to establish an *entente cordiale* with us.

If we only knew, too, when and where to put out our terrepathic tentacles! But we are so enmeshed in flesh, so sarcously conditioned that the purer they are the harder it is for us to get into *rapport* with them. This must not lead us to metaphysics, of which psychology has now far more reason to beware than physics ever had.

But seriously what is pure psychology? Is there and can there ever be such a thing? The answers to this question are flagrantly contradictory. Let us tab off a few of them.

The ultra-introspectors tell us that we must cut loose from physiology, and that to be pure it must be introgression or introversion. It is limited to the field of conscious awareness, and it emerges only as the result of a very technical and skilled process. When we approach the limitations of clearness and certainty we may refer our perplexities to "physiological processes" in general, but these are quite outside the field of true psychology. The purest of pure psychology would be bare of tropes and symbols, would transcend language itself, and be in fact nothing more or less than imageless thought, if there be such a thing.

It is on this that the Grail quest focuses, and not till this triply refined destination is found shall we have the true elixir *vitae animae*. Those who study the soul by somatic methods, or seek to gather psychic facts from animals, children, savages, the insane, social and business phenomena, etc., are not Simon pure psychologists but seek to enter the true

fold by forbidden ways. Only the ingrowing mind is truly psychological, and not consciousness but self-consciousness is the true muse for the psychologist to woo. This hypothesis, let it be freely confessed, has proved a very fruitful one and has made very material additions to science, but its limitations have become far more apparent as a result of the war. It is a movement which has passed its zenith anyway. Its fault was its fatal *ubris* that made it assume if not to be the exclusive method of psychology to be at least superior to all others in its methods and results.

(2) Its Nemesis was behaviorism, which went to the opposite extreme of attempting to evict consciousness entirely from psychology and make our science objective and descriptive. Like the Naples tropists it proposed a new nomenclature to be substituted for every process implying awareness. The prime data of psychology were secretions, hormones, excretions, assimilation and dissimilation, and other physiological processes. In addition to these come everything that can be called conduct, including not only acts but tendencies to action. Thus the stone which the introspective builders rejected became the chief stone in the structure of the behaviorists, and the pure psychology is consciousnessless. The soul is solely what it does in and with the body, and the physiological processes are all that we can study. Consciousness must be dethroned as a seductive hetera. Like the spirit of dulness that Pope invoked in his "Dunciad" consciousness, we are now told, is the basis of endless confusion and obfuscation, and it has been in short the evil genius of all who have studied the soul. Its business is to camouflage what really goes on in it. Thus its banishment will mark a new era.

(3) Psychoanalysis, too, impugns consciousness and finds the purest psychology in the unconscious; it makes consciousness a superficial manifestation of the real psyche. It has no hypothesis as to the relations of the body and the soul. It worked largely at first upon the abnormal, where nature and disease do their sad vivisections, but the center of gravity of its interest in the work is rapidly passing over to the normal. The mainsprings of all our psychic activities are found below the threshold. Psychopathology is chiefly used as a key to man's true nature, and finds certain subliminal mechanisms that eradiate from hunger and love, which it would restore to their true dominance in human life. Thus consciousness is only the froth or sillibub which overlays man's deeper instincts, individual and social. Even philosophers must be analyzed just as symptoms and delusions are, and they are not understood until we find in them the deeper unconscious ten-

dencies of their age, their nation and their race, as well as their own individual experiences, of all of which they are the voice.

But all systems of psychic life are deciduous leaves which are for a time agents of growth of the tree of man's soul but which fall off when their service to the larger life of the tree itself is accomplished. Consciousness is thus symptomatic and symbolic of the larger instinctive soul that makes health or disease and brings weal or woe. Its web is a new veil of Maya; the picture is only the curtain yet to be raised before we can see the real interior. Consciousness is only a medieval masque or play which presented an underlying moral so richly dight with circumstance and incident that it was a little hard to find. The pure psychologist here is the interpreter of what man thinks he thinks, feels, and does into a more primeval language of what he really thinks, feels, and does. Thus it is somewhat akin to

(4) Geneticism which says of the soul, as Bleek did of language, *Sie ist was sie wird*. From this standpoint the soul is hoary with age, it puts on and sloughs off customs, cultures, and civilizations but remains itself but little changed. Its progress though very, very slow is sure. It stresses in the modern culture of man and society the outcrops and survivals of the instincts of animals, children and savages, notes the influences which arrest and facilitate development and evaluates them accordingly, and is keen to discern racial traits and memories. Its muse is the *élan vital*, growth, or nisus of evolution, but whatever it is (Nicolai says it is God in His only true and essential nature) it impels things upward and onward. This, at any rate, is the divinity of geneticism. It confesses its obligation to Darwinism, its watchword is development, and it values the results of all schools of philosophic thought, although it refuses to be bound by or confined within any of them for its genius is synthetic. It conceives psychology as a bud, not as a flower and still less as a fruit. It would touch life at every point, it would gather material from every field for nothing human is without interest to it. It is suspicious of definition but hungry for facts, and it respects all methods only in proportion as they yield them. It has nothing much to give but vastly more to learn, and it realizes that finalities are yet afar off. Its chief interest is in description and observation, under control of conditions if possible, but it finds precious and still more numerous data where nature and life alone make the conditions, as e. g. in this war.

Mathematics may be pure or applied. Astronomy, one

expert in which says it has contributed nothing of practical value to man for the last two centuries despite its vast progress, is certainly pure if purity means uselessness. There is a very real distinction between scientific and economic geology, botany, and zoology. Paleontology has rendered but little service to man in this sense. In physics and chemistry the distinction is sun-clear. Both tendencies are strong, and the appeal of each often causes real conflicts in the minds of their devotees. Competent and impartial opinion is pretty accordant that in all departments of knowledge that make both appeals it is pure science that is not only most worthy but in the long run the most profitable, because many if not most of the great discoveries preceded and made possible the great inventions. Pure science is one the world over and transcends the differences of race, nation, temperament, and is accepted always, everywhere, and by all, for it is above race and parties.

How far is this true of psychology? Most of it that can be called scientific in any strict sense it took over from physiology, and it has added perhaps a few laws of memory and association. What more has it that can be properly called a purely scientific criterion *semper, ubique, et ab omnibus?* True it has collected masses of invaluable facts by observation, on e. g. animals, but we have no theory of instinct and are divided as to whether they are tropisms or really psychic. We have voluminous data on mental abnormalities, but no one has yet explained hysteria, and there is no generally accepted basis of classification of psychopathic disorders. In the field of child study there are countless observations, but no one has yet attempted even to bring the facts together and we only partly know what they mean, while finality is very far off. The same is true, though in less degree, in anthropology, where the same facts are differently interpreted by the different schools. Think, too, of the wreckage of theory that strews the path of the history of our science beginning with the Greek conceptions of the soul at the time when the foundations of geometry were laid down as they still remain! Even within our own memory English associationism has been reconstructed and supplemented by the doctrine of apperception. We hear little of the psycho-physic law which once occupied such an important place in our textbooks, for it has given way to Herbart's mathematical constructions. Some two decades ago two of our leading American psychologists insisted that suggestion was the keyword to unlock about all the secrets of the soul, explaining even originality by it. Hardly a single decade ago Kraepelin carefully elaborated the schematization

of psychic abnormalities which, although our best clinicians had accepted it, began to crumble and is now recognized as hopelessly inadequate.

Hering, Simon, and Loeb tells us that the central thing in the soul is memory. Schopenhauer and the modern voluntarists thought it was will, while Horwicz and other moderns galore think feeling or affectivity is the basis of all. Somatologists from the old localization studies of Ferrier down to Crile make the brain and cortical processes supreme. Frohschammer thought the imagination the root of everything, and Baldwin has lately urged that the ultimate test of truth itself is beauty and says that the psychologist must learn to know the world *sub specie pulchritudinis*.

In this country at some institutions psychology means chiefly mental measurements and their correlations; at others it is introspection; at others it is the study of the self; at still others geneticism, and elsewhere behaviorism. The fact is our science is like Milton's tawny lion "pawing to get free" from the soil in which it grew, or rather it is like Rabelais' Gargantua, gigantic but infantile, appropriating everything in its reach but not yet skilled in using anything because its body is still in the gristle and is only suggestive of the unprecedented power it will possess when it matures. Long before that we and our best work will be transcended and superseded, and we shall be recalled only as crude and ancient pioneers, or forgotten as a missing link; for just as surely as the Delphic admonition to man to study himself, or Pope's slogan that the highest knowledge of mankind is man, or Tennyson's line that self-knowledge, self-reverence and self-control, these three alone lead man to sovereign power, will social psychology, if the latest and hardest of all the sciences, ultimately be supreme over them all.

When its *Tag*, which is only dawning, comes on Haeckel's cosmic clock, the day which young men's visions and old men's dreams already begin to anticipate, will psychology be pure only in proportion as it is practical, just as the soul is a part of and not apart from the body. At any rate the more perfect psychology I dream of will be the acme of humanism. Every human institution will be evaluated by the sole criterion of how much it advances the kingdom of man. On this scale of worths education, art, literature, language, industry, trade, statecraft, law, curative and preventive medicine, philosophy—in short every human institution and invention of man and even the special sciences themselves will be weighed, for the only true and ultimate values in the world are psychological values.

In a sense the youngest mathematician is thus an antiquarian, for the foundations of his science were laid thousands of years ago, while the oldest psychologist is only a tyro or neologist, since his science is even now in its birth throes. It should be the organizer and synthesizer *par excellence*, while it is now the analyzer and the divider, separating co-workers into sects and parties. Abstract science ought at least to organize the brain, for, we are told, it always seeks the easiest way of comprehending the universe; but psychology, which should show men not only how to think but how to act and even how to feel, has as its ultimate goal nothing less than the organization of the whole of life, and hence if we accept pragmatic sanctions it is the purest of all pure sciences because and in proportion as it is the most practical of all, for its sole quest is the facilitation of the dominion of Mansoul in work.

(c) The war has brought us face to face with the problem of feeling with which psychologists had only just begun to deal in earnest, save by way of schematization. We have learned, for instance, that the all-dominant motive, all the way from the first possibility of war to the draft and on to the charge itself, is fear, for courage is only fear controlled and cowardice is fear yielded to. Even shell shock, which is four times as common among officers, who must not only be brave but must set examples of courage to their men, and which rarely attacks men in action, is only fear abandoned to. Anger and rage, if they come, can themselves by their sthenic reenforcement most effectively suppress fear. Nearly all these numberless hysterical phenomena are only individual and not racial, and thus the anxiety that causes them has fundamentally nothing to do with sex so that the Freudian explanation of them collapses, although most of the mechanisms remain valid and apply to their development and their cause, as Babinski, Eder, Smith, MacCurdy, and many others clearly show. Every experience with shock from childhood on affects temibility, and if we can find a test to eliminate panic starters psychology would achieve another triumph.

To the question whether, and if so how and toward whom, we should cultivate hate for atrocities, as in the cry "Remember the Maine" or "the Lusitania," we have no answer. Again, we have not yet evaluated the new attitude toward death which has arisen from the war, in which millions of the fittest to survive have fallen.

So, too, while creeds, rites, and sects have crumbled psychology should show how true religion can and should have a rebirth from the humbler but fundamental virtues of the trenches: loyalty, comradeship in arms and in danger, devotion

even to death to a cause that so far transcends individual life that it is gladly surrendered. These are the phenomena which we see in the unique religious revival that has inspired the *poilus* and has grown to such proportions in the French army and at home.

What is honor? Personal, which is defined as the religion of the soldier, and especially national, which the treaties of the Hague refer to but have never defined. How is it lost and reacquired? What is its true sphere and function, and how can it best be utilized?

So, too, of patriotism: Is there such a thing, is it a true sentiment, and if so of what is it composed? Can we draw a line between its defect and its excess in chauvinism and jingoism? How can it be cultivated? On what do racial attractions and antipathies rest, and is this correlated with successful interbreeding? It is feelings, emotions, and sentiments that are the dominant and determining factors in all these domains. Can psychology harvest all the vast crop of data which war gives before they decay and are forgotten, as feelings so soon are? "Out of the heart of the issues of life" might be the motto of war psychology, for pectoral or theumic phenomena constitute its very warp and woof, and its intellectual conscious phenomena are only laid-on patterns which camouflage what is going on under the threshold.

(d) We must not fail to garner the rich material in mass psychology and pathology offered by the war. Even more than the followers of Le Bon the school of Durkheim would have us believe that all great institutions, religion, language, myth, cult, and *mores* generally, were struck out not by individuals but by the collective folksoul under the influence of the calentures which characterize gregarious creatures when they get together, when the soul of each is expanded towards the dimensions of the soul of the group, thus deploying the higher powers of man. Now no human companionship begins to be so close as that of comradeship in arms, and in none is the individual so subordinated to the whole, like the cell in the larger organic unity. Here suggestion may become contagion, and rumor may grow beyond all bounds. There is the great problem, too, of the proper lines of censorship. Music and song, games and recreations, slogans, mottoes, watchwords, example and imitation are never so potent for good or ill.

All the psychological processes have their culminating expression in armies, which they weld into a solidarity which seems to be reversionary toward an antique type of mind and life that our age of rampant individuation has almost forgotten. Nicolai in his "Biology of War" tells us in substance

that consciousness of kind is the only true and living God, that the only true theology is anthropology, the only theocracy, democracy, and that if we could reinterpret the Divine and reduce it back into the human, of which it is really only a shattered projection, as Feuerbach and Comte vaguely try to do, we should realize that all men are, indeed brethren, that mankind is an organic unity and is itself the only true object of reverence, love, and worship, and then war would be impossible.

(e) Has not the war taught us in all these ways that the range of conscious awareness is limited and covers only a small part of the domain of psychology compared with the rôle played by the unconscious forces? Who that has argued with a maimed soldier, who felt either that he was down and out or else was of the type who assumed that he had done his heroic bit and that the world owed him a living without any further effort on his part to "carry on" and utilize all the powers he had left; or who that has cured a case of shell shock by making the victim of it understand his own latent mental processes and thus illustrated how consciousness is essentially remedial; or who that has observed the quick cure, surprising beyond all conception, effected on those malingering by discharge, by the end of the war, or even by being taken prisoner, can ever possibly doubt the potency of psychic processes, far outside the apperceptive focus, which the Wundtian school has made us so overemphasize! Even yet we know far too little of all those diseases that are mind-made and thus can be mind-cured.

How, too, about the so-called "adrenalin type" of soldier who, when he is fatigued to utter exhaustion, may be caught up and reenforced and fight on indefinitely because he cannot help it, as if he had suddenly tapped some great racial reservoir of energy, who can hardly remember afterwards what he did or what happened to him, even though he may continue to rehearse it later in dreams or in waking delusions.

Who doubts that unconscious factors were at least part causes of the war as well as of all that we call morale. The arbitrarily isolated and tiny sections of psychic life which we study under the arbitrarily controlled conditions of the laboratory must not give us a tonic cramp of apperception upon an object so small that we lose *einfühlung* for the larger perspective of life. Is it entirely commendable for psychologists to keep at their old tasks as if this unprecedented psychic storm had not swept over the earth, rather than respond to the call of these wild elements that are revealing so many new aspects of human nature because some of them are *Bewusstseinsunfähig?* It would seem that psychologists who had not lost

touch with life and mind would be more interested in this subject than in any other.

(f) As we have put far more psychology into the war than any other nation, and as we have led in injecting it into education and business, as witnessed by the efficiency movement, corporation schools and vocational guidance, we ought now to assume a new world leadership in this field. We took the laboratory movement from Wundt and now have more academic equipment for experimental psychology than all the rest of the world combined. We took the post-Wundtian introspection from Wurtsburg and have greatly enlarged the scope and enriched its results. The Binet-Simon method we took over from France and carried it far beyond the wildest dream of its founders. Behaviorism originated with Paulsen, Bechterew and other scientists, child study came from Preyer, the questionnaire method in schools from Lazarus, and psychoanalysis, which is barely heard of in our association, has a group of enthusiastic psychiatric disciples who have contributed many interesting cases but so far have done little that is constructive. We are still too pre-evolutionary to venture much into the field of heredity and eugenics and to evaluate hunger or sex, the two taproots of psychic life, and we make almost as little use of anthropological as we do of psychopathological, social, and religious results.

Thus everything summons us to a very new, serious, and deep and wide orientation, and also to a new world leadership and a higher quality of originality. Only in certain limited parts of the field has our psychology escaped the mechanical and racial *tendenziös* influences which only the exact sciences are able to transcend. Wundt was a physicist and physiologist before he was a psychologist, and he has never escaped the obsessions of physical methods, instruments, and even goals. Thus he to some extent has represented a dehumanizing *Kultur* trend that his more humanistic *Völkerpsychologie* has not entirely compensated for, while his successors with their hyperanalysis, which makes almost any synthesis never so hard although never so needed, and their hypermethodic ways tend to discredit the insight and intuition which is the mainspring of all creative evolution and which, as Plato said, students of the soul must never lose sight of. Must we then not look more critically at and accept with increasing reservations in the future certain influences of our Teutonic co-workers, even though we may and ought to forgive the ninety-three of them who signed the notorious manifesto of October, 1914, which Nicolai, a professor of physiology in Berlin, calls "the most notorious scandal of science."

What we need and want above and beyond all things else are psychological facts in whatever field and by whatever method, and next their evaluation and especially their synthesis, for psychology ought now to enter upon a new synthetic stage. Has not the time also now come to relegate either to the past of metaphysics or to the future as premature such unanswerable problems as those of parallelism and interaction, the Lange-James paradox, the ipsissimal self, freedom and necessity, and apply our energies to problems that are today capable of solution?

(g) For one I do not find the true substitute for war in James or any of the six candidates lately brought forward but rather in the conquest of nature. Man has subdued animals, domesticated himself in civilization, and is now learning to subject to his use the vast powers of inanimate nature at such a rate that a French physicist estimates that each of the one and one-half billion people on the earth today controls eight times the energy that he did a century ago. The earth, we are told, could soon be made to yield some thousands of times as much food as now, and could support a thousand times its present population. The forces of nature and of life are practically inexhaustible, and to command them by science and invention is now the real struggle for existence. A term translated and interpreted as *Kampf ums Dasein* by Darwin's followers had the disastrous consequences of Nietzschēism and Bernhardiism, while the other half truth of Darwinism—cooperation—stated with equal one-sidedness by Kropotkin, Novikoff and other Russians has led to an equally disastrous Bolshevism. Compared with this long struggle of man to dominate nature even this vast war is but an alley brawl, although if all its energies had been turned toward the research and control of the sources of energy, instead of regressing the kingdom of man might have been materially advanced.

With this recent eight-fold augmentation of power should go responsibility, for otherwise what guarantees have we that it will be used for human weal rather than woe, that it may not be for the advantage of men like Hubert, who invented asphyxiating gases, rather than for men like Emil Fischer, who has made sugar in his laboratory and in so far has taken a momentous step to free man from his dependence upon plant life for sustenance. Such safeguards it is up to psychology in its department of morality to erect.

Again, the war has caused an unparalleled outburst of psychic energy. In less than two years this most peaceful of lands has created four million soldiers and equipped them, made all their accoutrements and sent half of them across

the sea, and turned the scale of victory against the strongest military force in the world, one hundred years in the building. We have speeded up almost every kind of industrial work, and many of our soldiers have even learned French "over there" at a rate that puts to shame the slow pace of our schools. All this shows anew that man's soul, like nature, honors far vaster drafts upon it than we have hitherto presumed, and that there are new and unused capacities that should be utilized for education and should be explored by psychologists.

As for man in general the substitute for war is to control nature, so for psychologists it is to know and control psychic energies, which not merely cause war and peace but make all our lives their sport. We have lived to learn how the psyche controls health and disease in the individual, but we have yet to learn how the collective psyche makes or mars all the processes of civilization. Here, then, we have a new call. The argument for it runs roughly thus:— We were the first great democracy or government of, for, and by the people, and if the voice of the people be in any sense the voice of God a true democracy tends toward becoming a real theocracy. Now we have done much to win the great struggle which has made the world democratic, and thus we have come to a new world leadership. The success of this democratic leadership depends upon education, and practical and sound education is based on the natural powers, instincts and aptitudes of children, youth, and adults. Therefore the future of the world depends as never before on what human nature is really found to be. On this the entire future of the race is staked, and to thus rightly conceive and develop human nature is the special task of psychology and should give it a new priority in the world.

Thus there is a large sense in which the race is coming to depend as never before upon the truth and adequacy of our appreciation of the fundamental nature, needs, and possibilities of man, how to know him aright and to bring him to an ever more complete maturity.

DURATION, ENERGY AND EXTENT OF REACTION MOVEMENTS — SIMPLE AND FLYING REACTIONS

By Frank Angell

The following investigation is in continuation of the "Preliminary note" on reaction times and of the description of the "trigger" reaction-key in an earlier number (Vol. 22) of this magazine. The key described there was a self-registering spring-balance which gave the extent and strength as well as the time of the reaction movement. In addition the key could be set for different initial tensions so as to show the relation of variations in tension to extent and time of the reaction movement.

With the one reagent employed it was found that there was no constant ratio existing between the length of the reaction-time and the strength (and length) of the reaction-pull, and that consequently the increase in reaction time found for increased initial tension was probably owing not to the time spent in generating stronger reaction impulses, but to the increased drag of the key on the reaction movement.

In the meantime Isserlin's interesting and meritorious analysis of curves of simple voluntary motion [1] appeared which besides taking up the question from the physiological side, has covered considerable of the ground projected by the writer in the 'preliminary note.'

The essential feature of Isserlin's work is the analysis of the curve of the reaction movement traced on a kymograph, the movement being the flexing in a horizontal plane, of the first joint of the fore-finger. For this purpose the finger was made fast to the lower side of a light aluminum wheel, vertically pivoted, the axis of the wheel being in line with the pivot of the joint. Around the rim of the wheel ran a fine silk thread to a pointer sliding on a vertical track in contact with the kymograph, the thread being held taut by a light spiral spring. Adding to this a fork and an Ach card-changer, Isserlin read directly from the tracing on the drum the time, form and extent of the reaction-movement. The reaction-time in this arrangement was read from the space between the mark of the simulus (card-changer) and the point where the reaction curve left the horizontal line on the drum.

[1] Max Isserlin: Ueb. d. Ablauf einfacher willkürlichen Bewegungen. Kräpelin's Psy. Arb. B. VI, Heft 1.

It would have perhaps been better if the time of reaction had been given by a time marker so as to show any error which might come from the stretching of the thread in the reaction movement. Isserlin states that this error was 'negligible' as measurements showed that the pointer passed through practically the same space as a point on the periphery of the wheel. It would have been well to give these measurements and likewise the tension on the thread for maximum motions. If, as is probable, the measurements were taken by slowly moving the wheel around certain angular distances and noting the corresponding spaces passed over by the pointer, the results would hardly be normative for the sharp, quick sweep of the finger in the actual reaction movement. It is certainly not easy to see how a thin silk thread, not far probably, from a meter and one-half long, passing around a wheel and over two pulleys and pulling against the friction of the track and against a spiral spring, should not have been stretched by a sharp movement of some 6 or 7 centemeters.

The part of Isserlin's work with which we are more immediately concerned is entitled "Die Einstellung in ihren Beziehungen zur Bewegung" (S. 86) and as the title, implys, it is devoted to investigating the relation existing between the reaction 'attitude' on the one side, to the time and the forms of the reaction curve on the other.

Agreeing with Ach that the "problem" determines the way of reacting — that the distinction between muscular and sensorial reactions is the difference between the determination to react as quickly as possible and to react after obtaining a more or less full knowledge of the stimulus—I. finds the reaction curve a better index of the reagent's attitude than the reaction time; for while one may draw valid conclusions from the distribution curves of reaction times, each single reaction curve is a legible index of the attitude which determines it. In the sensorial reaction the motion is slower and the curve flatter than is the case with the muscular form. Reagents whose curves show a strong development of motor energy are 'muscular' in their attitude; weaker curves come from sensorially 'set' reagents. The curves showing greater energy follow the shorter reaction times.

As before stated, the present investigation is a continuation of the work with the trigger reaction key to further inquire into the relations of length and strength of pull to other factors entering into reactions. In order to directly compare the results with Isserlin's the index of the key was connected with a lightly running ergograph pointer in contact with a Balthzar drum. Some little trouble was experienced with the cord

connecting the key index with the carriage of the pointer on account of its tendency to stretch with the longer pulls. Finally a fine steel wire (strand of a picture wire) was used, linked to a short length of braided silk line where the cord passed around a pulley on its way to the carriage. The stretching of the cord made no difference in the time relations of a reaction, as the beginning of the reaction movement was signaled by the break of a contact on the key; but it would have made a difference in the shape of the first part of the reaction curve. As the lightest pull in our experiment was 100 grams, there was more of a drag on the cord than in Isserlin's work. Consequently the pointer was adjusted at the beginning of each day's experiments and not infrequently, in the course of a day's series. The minimum pull of 100 grams came in part from the friction of the pointer carriage, but mainly from a spiral spring of sufficient strength to prevent backlash, with the long quick pulls; it also served to bring back the carriage to its starting point.

The stimulus was given from a telephone placed behind and a little to the right of the reagent. Beyond this came the Morse key which sounded the false stimulus occasionally employed. None of the apparatus save the reaction key and a section of the connecting cord could be seen by the reagents.

In the first month of experimentation four reagents were busied in the work, but those were reduced to two, partly on account of the time required to take down the reagents introspections, and work out the curves, but mainly because in experiments where the introspections are often developed, from a viva voce exchange of question and answer between reagents and operator, it was felt that the former must not only have had considerable experience in reacting, but must be well enough acquainted with the operator to be 'objective' in answering his questions — a condition which is by no means a matter of course as between students and instructor. That is to say, the two reagents chiefly employed, Miss Lanktree (L) and Mr. Coover (C), were thoroughly interested in the work and at the same time had acquired sufficient laboratory 'standing' to feel independent of any 'leading questions' which the operator might be unlucky enough to put to them. On the other side the operator was careful to avoid giving the impression that he expected anything from the reagents save the "plain unvarnished tale" or indeed that any 'tale' at all was necessarily expected. With G. E. Muller's reflections on the danger of "Vielfragerei,"[2] the writer is in hearty accord.

[2] G. E. Muller. Zur Analyse d. Gedachtnisstatigkeit u. d. Vorstellungverlaufes. S. 122.

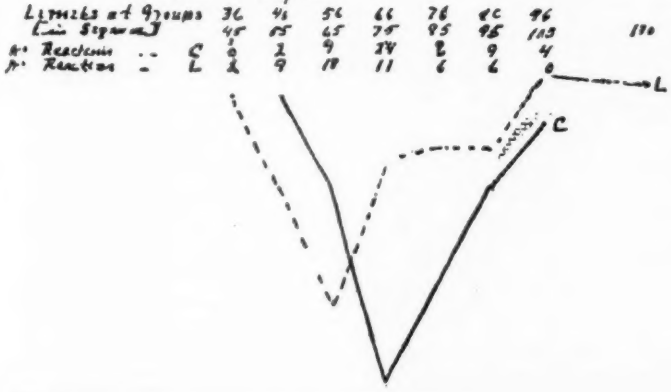
Duration of the Reaction Movement

The first part of Isserlin's work is taken up with the problem of the rebound or backlash (Rucktoss) from a quick movement, but into this matter we cannot go, because in our experiments the course of the backlash was obscured by the tension of the springs against which the reaction-movement was made —a tension running from 100 to 2,200 grams. But this resistance serves to bring out in a stronger light a constancy in the duration of the reaction movement independent of its extent —a condition found by Binet and Courtier[3] in their investigation of hand writing and further developed by Isserlin who formulates it in a rule to the effect that the entire duration of movements remains constant for each person regardless of changes in its course.

In the writer's experiments enough reactions were taken with L. and S. to make possible a graphic illustration of this constancy in the time of movement by means of distr.bution curves. These Curves are given in Fig. 1.

Had the abscissa units been 5 sigma instead of 10, the curve of L. would have shown a second maximum near the mark of 90, due to slowness of movement resulting from unfamiliarity

Fig. 1



[3] Rev. Philos., 35.

with the key on the second day of experimentation. The results of the first day are not included as they were not gotten under the same conditions as the rest.

But in any case the results are noteworthy and unexpected by reagents and experimentor. Of C's total of 54 movements, 52 fall within a compass of 40 sigma, and of L's 53, 51 fall within the same limits. Looking at the results more in detail, one finds that 6 of L's movements falling on 4 different days, take 60 sigma. The measurements are probably not far from correct, for with the Balthzar drum running on high gears, the tuning-fork curves were spread enough to enable one to read thirds of 0.01 sec. with ease. Consequently the actual times must have been between 59 and 60 sigma.

But these motions lasting 60 sigma vary in extent from 6.5 to 20 millimeters. Moreover the initial tension of the springs against which the reaction took place varied from 200 to 700 grams; the strength of reaction-pull ran approximately[4] from 450 to 1,250 grams, and the reaction times ranged between 97 and 157 sigma with an average of 119 and a. m. v. of 14. In the case of C. we find that 24 reactions lie within the duration limits of 60 to 70 sigma. They were taken on 7 different days against initial resistances of 100, 400, 600, 700 and 850 grams, varied in extent of movement from 9 to 45 millimeters and in strength of pull from 300 to 1,925 grams. The reaction times for 21 (for various reasons three could not be read) ranged from 113 to 313 sigma with an average of 159 and a m. v. of 23.

It is obvious that if there is any correspondence existing between the attitude of the reagent and the reaction-times, there is none indicated between attitude and duration of movement. As a matter of fact, the various instructions given — "React energetically," "React with a short sharp motion," "React naturally, as quickly as possible,"— are not reflected in the duration of movement so that perhaps in it we have the nearest approach to a straight physiological factor that enters into the reaction process. Indeed, these results remind one of Sherrington's diagram of the "scratch reflex" of the spinal dog where variations in the intensity of stimulation are answered more by variations in the extent and velocity of the flexion movement than by changes in its duration.[5]

[4] The "approximately" results from 2 springs attached respectively to the key and the carriage. Hence the total strength of pull had to be determined by another spring balance—a procedure which allows a considerable play of error.

[5] Sherrington, Intergrative Action of the Nervous System, p. 73.

The Course of the Reaction Movement in Relation to the Instructions. Secondary Impulses

The instructions given to the reagents were mainly in two forms, viz.:

(1). React naturally as quickly as possible.

(2). React energetically as soon as your hear stimulus.

In the course of a sitting, 4 or 5 reactions under one form of instruction were succeeded by as many under the other, the appropriate instruction being given emphatically, with few exceptions, before each movement.

A few reactions (not included in the quantitative statements of the preceding paragraph) were carried out by reagent C. under the instruction: "As soon as you hear stimulus, react quickly and gently."

For L. the instruction ran: "React shortly and quickly as soon as you hear stimulus." At the beginning other forms were given such as "React as quickly as possible with a short sharp pull," or "React as quickly as possible with a long, quick motion": but as interpreted by the reagents in the reaction process they were covered by the 3 forms given above — "short and sharp pull" working out as "quick and gentle" and "long and quick" as "energetic." But the change from a series with the instruction to "React naturally as quickly as possible" to "React energetically as soon as you hear stimulus" was a step of increased but not excessive difficulty, requiring usually a more elaborate preparation in the pre-period. "React energetically" supplemented "react quickly" in the reagents attitude but "react quickly" and "react as soon as possible" result in about the same attitude.

In the case of L. and C., the determination once made to react "naturally" or "energetically" was "set aside" ready for use and the reagents gave themselves up to the reception of the stimulus, trusting that the reaction movement would follow automatically — which was usually the case. But despite the automatic character of the reaction, the determination to react "energetically" made itself manifest both in quickness of response and extent of movement. Eighteen (18) of L's natural reactions taken towards the end of the investigation gave: median, 119,— average, 117 (m. v. 9). Thirteen (13) energetic reactions gave median 107.5—average, 109 (m. v. 8). For C., 23 natural reactions gave median 157—average, 166 (m. v. 33); 20 energetic reactions gave median, 146.5—average 150 (m. v. 21).

The reactions of L. and C. would hardly have fitted into

the traditional forms of muscular and sensorial reactions: L. in her reception of the stimulus was decidedly sensorial; she rarely responded to the false stimulus (click of an electric signal), and in the rare cases where a false reaction took place, there was no lengthening of the succeeding reaction-time. When a false stimulus was given, L. usually recognized it as "false at the time" whereas C. often reacted and then felt as if he had "reacted to nothing." Still it seems that L. reacts to the intensity component of the stimulus rather than to its quality: she notes repeatedly "the impact of the sound" and asserts she trys to make the appearance of the sound and the reaction movement coincide. Now her reactions were quick (natural 117, energetic, 107.5), quicker than those of the so-called muscular type. They were also automatic, i. e. the satisfactory apperception of the stimulus was followed, without effort by the appropriate natural or energetic reaction.

The course of C's reactions was towards the muscular form until checked by a false reaction. As an experienced reagent he quickly dropped from a full to a schematic apperception of the stimulus. The analogy he used to explain this was that of "significant," versus proof-reading. False reactions threw him back into a minute apperception of the stimulus with an increase of about 100 sigma in reaction-time. Usually he carefully imaged the reaction movement and then "set it aside." In case of a genuine discriminative reaction (between real and false stimulus) he notes the reaction movement is automatic. In the course of time he comes to take a spatial property of the stimulus as determining factor. (The telephone was placed slightly behind and to one side of his right ear). This factor he characterizes as "a knowledge of the form of the stimulus as articulated and referred to the right ear as recipient organ." The reaction movement was automatic, but "the extent did not sometimes correspond to the intention of the pre-period." But C's reactions, even when his apprehension of the stimulus was most abstract and therefor nearest the muscular type were longer than those of the sensorially reacting L.

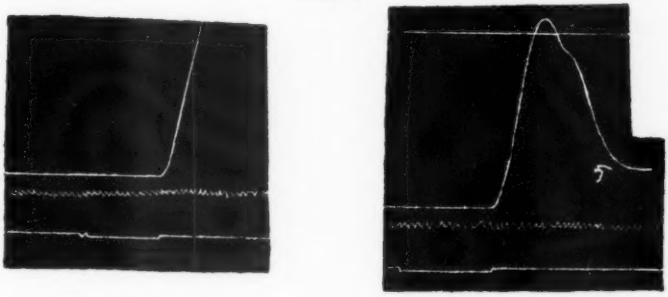
In order to analyze the reaction curve in successive moments (1/100 sec.) of its course, the kymographic tracings were placed in a reflectoscope and their images, magnified about 7 times, were reflected on drawing paper. These enlarged reflections were then carefully traced with lead-pencil and the tracings taken to the drawing-board for measurement.

The curves are not directly to be compared with those of Isserlin on account of the rapidly increasing resistance applied in our experiments, though despite this difference there is

considerable likeness in the typical curve of both investigations.

Typical forms of the reaction curve are given in Fig. 2.

FIG. 2.



R. t. 164 sigma
Duration of motion — 100 sigma.
Length of pull, 38. 5m. m.

R. T. 184 sigma
Duration of motion — 115 sigma
Length of pull, 47m. m.

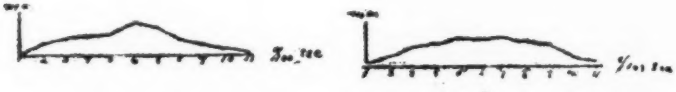
By means of the enlargement, one is able to measure with a fair degree of accuracy the distances passed over by the reaction movement in successive hundredths of a second. For the two curves above distances are:

1-100 Sec.	M. M.
1.	0.27
2.	2.08
3.	3.70
4.	4.50
5.	5.00
6.	7.60
7.	6.10
8.	3.90
9.	2.70
10.	1.40
11.	0.55
	37.80

1-100 sec.	M. M.
1.	1.1
2.	2.4
3.	4.5
4.	5.0
5.	6.4
6.	6.2
7.	6.9
8.	5.7
9.	5.2
10.	2.7
11.	1.2
	47.3

The discrepancy between the direct measurement on the length of pull and the form of component pulls comes from the rounding out of decimals.

The graphs for the above figures picturing the distances pulled as function of successive units of time (1/100 sec.) are given in Fig. 3.



But while Fig. 2 shows the typical form of reaction curve for all reagents, there were not intrequent deviations from this form arising from causes in part physical and in part physiological. In a few cases, the reagent seemed to be dissatisfied with the extent or strength of pull and voluntarily pulled again. More frequently, however, the curves showed a sharp break in their course, resulting from a period of total rest or even period of relaxation followed by a secondary reaction not noticed by the reagent. Thus reagent L. after the instruction to "React energetically as quickly as possible" gave a reaction movement in 96 sigma which ended after a period of 60 sigma, passing a distance of 9mm with a pull of 475 grams. This was followed by a moment of rest lasting between 10 and 20 sigma when a new impulse was started which lasted 57 sigma, passing over a distance of 16mm with a pull of 900 grams. The reagent's introspections give no indication of knowledge of this double pull except that it was "thought the pull was small." Similarly C. after a reaction time of 160 sigma, pulling for 68 sigma over a distance of 35 mm, paused for 20 sigma and then pulled 10 mm further. These secondary reactions, following the primary after an interval of from 10 to 30 sigma remind one of the "incremental reflex" in Sherrington's experiments[a] though it is to be remembered that these movements in our experiments met the uniformly increasing tension of a spring as well as the energizing variations in the muscle.

Whatever the nature of these secondary movements, they raise the question of the length of reaction-time when the reacting member is already in motion. More specifically,—What will be the effect on the time of reaction if the stimulus acts after the pulling motion has begun?—a question which also follows logically W. G. Smith's investigation of "Antago-

[a] *Op. c.* p. 75.

nistic Reactions"[7] where it was found that for some reagents, in the ordinary break reaction, the antagonistic effect ranged from 40 to 50 sigma. To answer the above question, the reagents were directed to draw themselves together as usual at the ready-signal, next to begin a gentle flexion of the reacting finger at the command "Pull"! and finally to "react as quickly as possible" when they heard the stimulus — these directions being repeated for each reaction. But it soon became evident that the ready signal would have to be dropped as it seemed to serve as a stimulus for which the preliminary pull was the answering reaction. Accordingly the starting of tuning-fork and drum came to serve as signal, after which the command Pull! was given, followed about half a second later by the stimulus from the telephone.

A difficulty which at first appeared serious resulted from the conflict in consciousness of the direction to begin a gentle pull and then follow it with a quick reaction to the stimulus: the intent to give a gentle initial pull acted as a strong distraction from the motive of the reaction movement proper in the earlier trials of the experiment. But surprisingly soon the preliminary pull became a matter of mechanical routine and seemed to exercise no distractive influence whatever. But to say that the motive for the preliminary pull had become inoperative because not noticed would be going too far. For an incident which took place on the 7th day of the experiment showed clearly that a state which the reagent through repetition had ceased to notice might become operative in opposition to the reaction.

On this day the operator started the drum as usual, gave the command Pull! and sounded the stimulus, neglecting however to start the tuning-fork, the outcome being that the reagent failed to react altogether. He did not know that he had not reacted, did not know that the tuning-fork had not sounded: he simply felt that something out of the way had happened but could not tell what. Inquiry showed that it was not a case of suspension of movement through surprise, but through the lack of one element of the reaction motive which the reagent had long ceased to notice. The next day, under like conditions, the reagent responded to the stimulus, though not aware that the tuning-fork was at rest: his only comment was that the interval seemed very short. In general, for most of the flying reactions, after the preliminary flexion had become mechanical, the command Pull! seemed to serve merely as a ready signal for the stimulus.

[7] W. G. Smith, Antagonistic Reactions. Mind, N. S. XII, 47. Vid. also Judd, McAllister and Steele, *Psy. Rev. Mon.* Sup. I, 131.

The reagents for this part of work were Coover (C.) who had served in the earlier stage of the investigation, W. Root (R.) who had shown himself a careful and dependable observer and the writer (A.).

TABLE 1

SIMPLE AND FLYING REACTIONS FOR REAGENTS C., R. AND A

Date	*Simple Reactions* Number	Average (σ)	Median (σ)	*Flying Reactions* Number	Average (σ)	Median (σ)
REAGENT A.						
Feb. 19	10	178	155	8	127	116
" 21	8	210	220	8	151	145
" 23	10	186	191	10	106	106
" 26	10	157	177	10	108	104
March 6	5	188	178	7	146	144
REAGENT R.						
Nov. 25	4	168	160	10	117	114
" 27	6	169	165	4	119	119
" 29	7	163	161	7	170	140
Dec. 6	..	...	..	6	117	116
" 8	6	168	165	9	113	114
" 11	7	153	150	7	114	115
" 13	4	149	146	10	127	124
REAGENT C.						
Jan. 19	7	171	160	8	125	125
" 22	8	155	153	8	116	118
" 24	11	148	155	9	123	125

TABLE 2

TEN QUICKEST REACTIONS—SIMPLE AND FLYING [S or F]

R.		*C.*		*A.*	
S	F	S	F	S	F
136	60	130	92	120	65
135	98	130	95	120	85
135	99	140	106	130	86
140	100	140	110	130	88
143	102	142	110	140	90
143	102	142	110	147	93
145	105	150	110	150	96
145	110	150	112	155	97
149	110	156	112	155	100
155	110	150	115	158	102

Table 1 and 2 give the figures for simple and flying reactions for each day's experimentation, and it shows almost, without exception that the flying reactions are quicker than the simple, even though moving against a stronger tension of the springs. The sole exception appears in the third series for reagent R, where two of the 7 cases gave uncommonly high figures. It

may be surmised that these two are the result of a conflict between the motives for the preliminary pull and the reaction proper such as frequently occurred at the beginning of this form of reaction. But in the absence of any note concerning them in the reagents introspections, they had to be taken up into the average. The median probably gives the more dependable value and this is smaller than the corresponding value for the simple reaction. The difference between the two forms of response is clearly brought out by the columns giving the 10 quickest movements for each of the several reagents.

A superficial inspection of the figures in this table might give rise to the impression that the flying reaction was a shorter variety of the shortened or muscular form, and that its relation to the latter was akin to that existing between the shortened and the complete reaction—the 3 forms thus making a sort of quantitative series of preparation or innervation of the reacting muscle. But opposed to this we find from the introspections that the flying reaction is more like the complete than the shortened form in most cases in requiring a strong and distinct voluntary impulse: in fact, the change from the slow preliminary pull to the sharp, hard reaction movement proper, is more distinct than the impulse which follows the apperception of the stimulus in many complete reactions.

Further we note on the physiological side, that the complete and flying reaction show a greater resemblance in the form of the kymographic tracings of the reaction movement. Isserlin[8] found that the kymographic tracing of the complete reaction slanted more from the perpendicular than that of the shortened form. This difference, which he considered characteristic was of course, due to the greater velocity of the shortened reaction movement in part or all of its course. A similar statement holds true for the relation between the flying and the shortened reaction movements: the flying reaction movements are relatively slower as was obvious from a casual look at the curves on the smoked paper. Taking the average velocities of the two forms of an hour's experimentation near the close of the work, when the flying reaction might reasonably be supposed to have arrived at a minimum of mental friction, one gets the following figures:

COMPARISON OF AVERAGE VELOCITIES (M. M.-PER 0.01 SEC.) OF SIMPLE AND FLYING REACTIONS

Reagent	No.	Simp.	No.	Fl'g.
C.....	7	3.5	7	2.3
R.....	15	2.9	11	2.7
A.....	9	4.1	10	3.3

[8] *Op. cit. S. 96.*

To what are these results due? The writer thinks that foi the most part, we have here to do with physiological factors Haycraft[9] has given quantitative expression to a well known fact viz.: that a reaction movement is discharged with greater velocity if started from a detent than from a normal condition of agonistic-antagonistic equilibrium. Now while in most shortened (muscular) reactions there is no external hindrance to the beginning of the movement, the agonistic and antagonistic muscles governing the flexion and extension of the reacting member are in more or less state of excitation as was shown not infrequently in our experiments in the not infrequent pulling apart of the contacts in the interval between the ready signal and the stimulus contraction. In the flying reaction the antagonistic muscle is already relaxed by the preliminary pull — a condition which would naturally bring out the most noticeable marks of these movements, viz: the slower motion and the quicker reaction.

Taking the differences between the medias for the simple and flying reactions we find that for A. the figures range from 34 to 85 sigma (5 series), for R. from 25 to 46 sigma (6 series) and for C. from 22 to 46 sigma (3 series). It is not improbable that these values represent the periods of antagonistic relaxation in simple reaction movements.

[9] Haycraft Upon the Production of rapid voluntary movements. *Jour. of Phys.* 23.

BOOK REVIEW

La Crisis de la Pubertad y Sus Cosecuencias Pedagogicas. By VICTOR MERCANTE. Buenos Aires, Cabauta y Cia, 1919. 437 p.

Senor Mercante's contribution to the psychology and pedagogy of pubescence, while particularly adapted to the educational problems of South America, contains a great deal of general information and offers suggestions of universal interest. His main thesis is that the age of puberty marks a crisis in the life of the individual and necessitates a school program adapted to its peculiar needs. Therefore he devotes his comprehensive volume to a study of the physiological and psychological traits which mark the onset of puberty, and to a detailed development of an educational plan based on this knowledge.

Chapter I, The Crisis of Puberty, approaches the problem from the broad biological viewpoint, and stresses the genetic interpretation of adolescent phenomena. There are a great many quotations from G. Stanley Hall and a number of French writers to substantiate the author's considerations, and two educational conclusions are drawn: 1. Education must not be contrary to evolution. 2. Whenever possible, education must favor evolution.

Chapter II is a very careful study of the organic changes during pubescence, and is a comprehensive summary of the literature on this subject, with additional data gathered in South American schools. There are numerous graphs and tables to show correlations and variations in height, weight, cephalic index, vital capacity, respiration rate, blood pressure, etc., at different ages of childhood and adolescence. The functional importance of all the glands of internal secretion in the production of the phenomena of puberty is emphasized, and the changes of voice at this period are noted. Especially noteworthy is the rather original statement that in the handwriting of the pupil the observant teacher has an excellent criterion for judging fluctuations in moods and peculiarities of temperament. The handwriting is considered to be a motor expression of the psychic state, and specimens are reproduced to show how it varies in the same student in childhood, pubescence, and late adolescence. The educational lesson to be drawn from the study of the physical nature of the pubescent child is the correct evaluation of games, sports, and physical training; and Senor Mercante enters a plea for the adoption of such exercises as tennis and especially football, which not only serve as an outlet for the over-abundant energies of youth, but have a great social and moral value as inculcating ideals of fair play and promoting the spirit of cooperation.

In the third chapter, a similar study of mental traits is made, focusing on sensation, perception, visual acuity, sensibility to pain, muscular sensibility, memory, attention, association, imagination, reasoning ability, etc. The important finding here is the indication that at this time in the life of the child the native tendencies make their first tentative appearance, so that this becomes a critical period for the discovery of vocational aptitudes. For this reason, as well as on account of the active nature of the pubescent boy and girl, which requires a constant motor outlet, courses in the secondary schools should include as much time in the laboratory and workshop as possible.

The fourth chapter, which is headed The Sentiments and Conduct During the Crisis, is a brief account of the sexual life in its relation to delinquency, vocational tendencies, and ecstatic experiences. One or two illustrations of adolescent love are cited, the danger of developing habits of self-abuse is touched upon, and the relatively high degree of sickness but low mortality prevalent among adolescents is mentioned.

Chapter V is an exposition of the necessity of an integral system of education at the pubescent period, which shall be as plastic as possible, and allow the child every opportunity for individual development. The cultural subjects are not to be discarded, but they are to be taught with a view to social, moral and aesthetic interpretations, while vocational training and physical education must assume a vital role in the curriculum. Chapter VI deals with the practical problems which are confronting the educators of South America in their attempts to reform the school system in accordance with these aims.

Chapter VII describes the ideal program for the secondary school, gives a plan of the correct type of building to be used, and outlines methods of teaching. Co-education is discussed, and meets with the author's approval because it foreshadows the social situation of adult life, but different curricula, adapted to sex differences, are insisted upon. The aim of education is defined as the development of the ability to meet the economic and social situations of life, and the building up of aesthetic appreciation and moral idealism.

Senor Mercante's work shows an extensive acquaintance with the literature of France and America on the subject of adolescent psychology and pedagogy, and a thorough comprehension of the work done by others along this line. But his chief contribution is his insistence upon an educational system adapted to the physical and mental needs of the pupils, and his appreciation of the aesthetic and idealistic functions of the school, as well as of its economic and social purposes.

PHYLLIS BLANCHARD.

BOOK NOTES

Papers on Psychoanalysis. By ERNEST JONES. New York, William Wood and Co., 1919. 715 p.

This book has been out of print for some three years on account of current events, but it has now been thoroughly revised and extended. One chapter of the original twenty has been omitted and twenty-one new ones have been added, eleven of which have not previously appeared in English. The papers deal with education and child study, dreams, treatment, general papers, etc.

Since the book was first published in 1913, two important events have occurred: (1) the abandonment of the principles of psychoanalysis by Jung and some of his Swiss pupils, and (2) the war has taken place, the opportunities of which have not been neglected by the opponents of Freud's work who have even gone so far as to say that anything in German must be wrong.

The author is probably, on the whole, the ablest and best informed of all the disciples of Freud who write in English. His loyalty to Freud is intense, and many will think that he does very scant justice to the views of Jung and even Adler. Strong as he is in his field and wide as his reading has been, interesting and acute as his special studies have been, and many as are the amplifications of Freudian views which we owe to him, it can hardly be said that his mind is creative or that any new departure of essential value is due to him. Wise as his scholarship is in his own chosen field, it is to be regretted that, like all the Freudians, he lacks a thorough grounding in the principles and history of normal psychology, whether laboratory, genetic, behavioristic, or comparative.

Autobiography of an Androgyne. By EARL LIND. New York, The Medico-Legal Journal, 1918. 265 p.

This book is inscribed to Nature's step-children, the sexually abnormal by birth, in the hope that their lives may be rendered more tolerable through the publication of this autobiography.

In the Introduction by Alfred W. Herzog, M. D., a member of the New York bar, we are told that the publication of the book is justified by its medico-legal bearings. We are also told that the author is now in his fifth decade but looks considerably younger, that his language is carefully chosen and shows much polish, his manner gentlemanly though somewhat embarrassed, his voice a little hoarse, that he is vain, and that he "is of a woman's soul in a body which he believes to be one-third female and thus only two-thirds male. He seems to glory in his feminine traits." Dr. Herzog was first assigned to edit the book, but on second thought believed it would serve its mission best unedited. We are told that the author really gives a psychoanalysis of himself without attempting to do so.

The writer of this notice has not read this book, but if it is all that is claimed for it, it has value for the psychiatrist as well as for the doctor, and it is said that it is a psychological study very worthy of careful analysis.

Kutenai Tales. By FRANZ BOAS. Washington, Government Printing Office, 1918. (Bureau of American Ethnology, Bulletin 59.)

Many of these tales were collected by the late Alexander Francis Chamberlain and were told to him by two men who had spent some time among the Blackfoot Indians. The tribe has been carefully studied, and the tales are printed here both in the vernacular and in English with an elaborate and valuable vocabulary, filling more than seventy pages.

Love's Way. By ORISON SWETT MARDEN. New York, Thomas Y. Crowell Co., 1918. 309 p.

We have interesting homilies on The Greatest Thing in the World, Making Life a Song, The Dream of Brotherhood, Work and Happiness, Training the Child, Survival Value, The Harmony Bath, Love Letters from God, Heroism at Home, etc. The book abounds in incidents and anecdotes, and attempts to cultivate a warm Christian spirit.

The Beginnings of Science. By EDWARD J. MENGE. Boston, Richard G. Badger, 1918. 56 p.

The purpose of this volume is to set forth in untechnical language the relations between philosophy and the laboratory sciences. In successive chapters the author discusses biological and psychological laboratories, genetics, metaphysics and epistemology, logic, the present status of evolutionary philosophy, the theories of evolution, vitalism, the ideal, and authorities. He also gives a summary of his ideas and a list of suggested reading.

Prussian Political Philosophy. By WESTEL W. WILLOUGHBY. New York, D. Appleton and Co., 1918. 203 p.

The chapters are: American Political Ideals; The German Weltanschauung; The Prussian Theory of the State, of Monarchy; Prussia's Constitutional Theory, and System; Tendencies Toward Ministerial Responsibility in Germany; Propaganda; and Conclusion.

Thrift. By ORISON SWETT MARDEN. New York, Thomas Y. Crowell Co., 1918. 92 p.

Thrift, we are told, is the foundation of all greatness and is a safeguard for the future. We must be able to finance ourselves. The art of saving is the art of wise spending. Every man should have an idea of owning a home of his own, and should avoid sailing under false colors. Spendthrifts of time and energy are rebuked. The saving habit, we are told, is a character builder. The lesson of this book is wholesome and timely, and is apparently addressed to the young.

Report of the Massachusetts Commission on Mental Diseases. For the Year Ending November, 1917. Boston, Wright and Potter Co., 1918. 286 p.

Help When Tempted and Tried. By JEREMIAH ZIMMERMAN. Boston, Richard G. Badger, 1918. 165 p.

These are a series of practical homilies upon seventeen themes: Temptations, Guarding our Thoughts, Prejudice and Abuse of the Tongue, the Fear of Sin and the Antidote, Selfishness and Intemperance, Ideals and Character, Need of Self-denial, Temptations to Dishonesty, Will Power, Self-murder, Power of Prayer, etc. It is an excellent handbook for serious young people and their parents.